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RATIONAL INDUCTION

AN ANALYSIS OF THE METHOD OF SCIENCE AND PHILOSOPHY

By

HOMER H. DUBS, Ph.D.

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TO

MY FATHER AND MOTHER

PREFACE

This book is an attempt to attack the problems of knowledge: What is knowledge, and how is it to be justified? It is obvious that these problems are fundamental to all philosophy and science. While the problem of the nature of knowledge has received a great deal of attention by philosophers, under the name of epistemology, the logically prior problem, How do we know when we have attained to knowledge? has never been adequately treated.

The logic of today, following the lead of Kant, consists largely in analyses of the *nature* of knowledge, studies of the kinds of judgments and concepts; although there is no explicit logical method whereby the correctness of an analysis can be established. Philosophers have merely made their analyses and left to the reader the problem of judging uncritically whether the analysis is well or ill made. Again, logicians, following the lead of Francis Bacon, have sought for methods of *discovery* or investigation rather than for methods of proof.

When Kant by his Copernican revolution revivified philosophy and inaugurated the brilliant period of German idealism, it was by the discovery of a new logical method. His successors have agreed that Kant was somewhere mistaken, although they have utterly disagreed in what respects he erred. It is now even doubted by some whether the transcendental method has any value at all. The present confused and uncertain state of philosophy has led some thinkers to look for another revival of philosophy through the discovery of a new method.

Perhaps it may be possible to bring about another Copernican revolution by turning from the problem that Locke and Kant placed in the forefront of logical discussion, What is the nature of knowledge? to the prior problem, How is knowledge to be justified? It is a thesis of this book that methods of investigation are many and lawless, whereas methods of proof are one and law-abiding. Consequently, the fundamental problem of philosophy and science is that of discovering: not a method of investigation, but a method of proof. This book will seek to investigate the traditional methods of proof, and to present a method which, while it is one of the oldest of all, has nevertheless been almost entirely overlooked by logicians. After more than two thousand years of philosophical and scientific thought, in which time practically all possible fundamental concepts have appeared, it is improbable that any radically new methods will be discovered. What is needed is not a search for something at present unknown, but a careful and exact examination into the nature of those methods of proof that have produced good fruit, and a scrutiny of the logical problems that they raise, in order to reformulate the method of inductive and deductive proof.

The method here offered is very ancient and widely used by modern science, but it has not been recognized as veridical by philosophers. It can be described schematically in a few words, but in its concrete application it is very complex. It is this complexity in simplicity that has perhaps blinded logicians to its true nature; while, at the same time, those scientists who have successfully used it have been too engrossed in their work of discovery and too little acquainted with the logical difficulties involved to give a clear statement of their own method. This method shows its power by solving some of the most perplexing problems of philosophy, in particular, by pointing a sure way out of fundamental skepticism. A few of the solutions thus rendered possible are presented in the third part of the book. For those who have not the time to read the whole book, a grasp of its central thesis may be secured from a reading of chapters i, iv, vii, and viii, especially chapter vii. The first part of the book clears the ground for this thesis and the third part works out some of its implications.

My inestimable obligations to the various writers, past and present, upon the subjects herein discussed are partially indicated by the numerous references found on the succeeding pages. Professor E. A. Burtt has read the manuscript, and has given me the advantage of his criticisms. I am also indebted to my wife, whose suggestions dot the pages of the book.

Homer H. Dubs

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PART I

THE URGENCY OF THE PROBLEM OF KNOWLEDGE

CHAPTER I

THE PRESENT STATE OF PHILOSOPHY

> Whether the treatment of that class of knowledge with which reason is occupied follows the secure method of a science or not, can easily be determined by the result. If, after repreated preparations, it comes to a standstill, as soon as its real goal is approached, or is obliged, in order to reach it, to retrace its steps again and again, and strike into fresh paths; again, if it is impossible to produce unanimity among those who are engaged in the same work, as to the manner in which their common object should be obtained, we may be convinced that such a study is far from having attained to the secure method of a science, but is groping in the dark. In that case we are conferring a great benefit on reason, if we only find out the right method, though many things should have to be surrendered as useless.—IMMANUEL KANT, *Critique of Pure Reason*, Preface to the second edition.

§ 1. These words of Kant were a stricture upon the philosophy of his own generation, which had failed to reach any sure and undoubted conclusions; but they are far more a stricture upon the philosophies of our own generation. Today there is even less unanimity among philosophers than there was in the time of Kant or in any preceding age. The various schools of thought that existed in the time of Kant are still represented in the intellectual world; in addition, we have a great many more schools, which sprang from Kant and his successors. One feature of the thought of our age is the revival and recasting of earlier philosophies—neo-Hegelianism, neo-Fichteanism, neo-Kantianism are among the strongest of the tendencies of the time, besides restatements of the philosophies of Spinoza, Leibnitz, Berkeley, Locke, Plato, and others. Furthermore, there are cross-fertilizations of one classic philosophy by another, and so there have come into being most of the possible combinations of thought. Even where philosophers have grouped themselves into schools, the individual members of the philosophic schools are rarely found to agree with one another. One would think that it is considered a disgrace, as showing lack of originality, for any philosopher to agree on too many points with any other—so various have been the disagreements even among those who profess to belong to the same school! The desire to be original seems to have passed into the desire to differ, at some point at least, with every other philosophic thinker. The result of this confusion has been that almost every philosophic writer of standing has become a school by himself, although on more general matters he may agree in the main with one of the many contemporary schools of philosophy. In ancient Athens the existence of the four warring schools resulted in doubt as to the possibility of knowledge, until skepticism and its twin sister, dogmatism, conquered the ancient philosophic world; how much more is skepticism a natural result of our more confused age! How philosophy suffers by a comparison with those subjects of investigation which have separated themselves from philosophy and have become sciences! We find conflicts there also, but conflicts only about the newer and less certain matters; whereas there is agreement among all who have mastered a science that certain truths have been established, so that progress is possible. Philosophy, on the other hand, is compelled to make a continual return to its starting-point in the endeavor to gain a sure foundation.

Indeed, contemporary philosophy gives much ground for skepticism. Since Kant founded the "critical philosophy," his critical skepticism has become the fashion; philosophers have overtly tried "to become aware of and to doubt all preconceptions."[1] Kant's own philosophy was one of the first to fall beneath criticism, in spite of his great authority and prestige; the "critical philosophy" was not sufficiently "critical"! The philosophies which have been founded on its ruins have in turn been criticized, until today it may truly be said that there does not exist any philosophy which has been established so securely that it is beyond the reach of destructive criticism. The offensive powers of philosophy have developed so much more than the defensive that today there is no fortress of philosophy which can be said to be secure. In the conflict of philosophies, the attacking forces have everywhere been successful, so that while they have succeeded in overwhelming the enemy, their own strongholds have likewise been taken! So common has been this experience that keen-sighted thinkers have even suggested that philosophy "has to renounce . . . . ancient ideals of rigor and finality,"[1] and even the most rigorously logical of philosophers are ready to admit that ultimate problems do not admit of clear-cut demonstration—yet philosophy is supposed to deal pre-eminently with just those ultimate problems whose solution is thus given up!

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[1] F. H. Bradley, *Appearance and Reality*, p. xii.

§ 2. It is not difficult to locate the reason for this lack of any consensus of opinion among competent philosophers and for their failure in establishing even one impregnable position. The process of "doubting all preconceptions" has gone so far that it has become evident that every philosophy necessarily assumes some preconceptions, and yet no way of establishing preconceptions has been found. Before we can attempt to be sure of even the least of philosophic truths, we must first meet and solve the problem of knowledge. Otherwise our philosophy is likely to suffer the fate of all other philosophies—while we are elaborating and strengthening its superstructure, we shall find that its foundations have been undermined. If any philosophy pretends to be true, or to contain any knowledge whatsoever, it must first determine under what circumstances we can be sure that we have knowledge. This is the fundamental problem of philosophy—not the problem of how knowledge is obtained. When we ask how knowledge is obtained, we assume that already there is knowledge, and are inevitably led into psychological investigations dealing with the method of learning or becoming aware, although we have not yet determined how we can be sure that our psychological or philosophical analysis of the method of gaining knowledge has been of the sort that can give us any certainty at all! All investigations into the method of knowledge have assumed that they know how to recognize knowledge before they began their investigations. If challenged on this assumption, they might reply that "ultimate problems do not admit of demonstration"—thus evading the question; but thereby they would have to give up the critical method of "doubting all preconceptions," and philosophy would become a dogmatic instead of a critical discipline. The fundamental problem of philosophy, and likewise of all science, is not the problem of the method of knowing, but the problem of certainty—how can we be sure that this piece of suppositious knowledge is real knowledge? Is there such a thing as certainty, and by what marks or by what method can we tell when we have reached certainty? With this problem we have at last reached the deepest of all philosophical problems, the one which must be solved before we can proceed to erect any secure superstructure.

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[1] W. James, in *Mind*, N.S., XIV, 191.

§ 3. In attempting to meet this problem of certainty, we may well avail ourselves of a distinction made long ago, almost at the beginning of philosophy. In the *Meno* and *Republic*, Socrates distinguishes between knowledge and opinion. In the *Meno* he gives an example of what he means by knowledge—it is a demonstration of what would be the side of a square containing twice the area of the original square. Knowledge is that of which there is demonstration. Socrates extended this idea of knowledge from mathematics to ethics and metaphysics, and sought to make philosophy a science, a discipline that could be taught just because it was knowledge, and that would have the logical power over men's minds which is possessed by mathematical demonstration. In contrast to such knowledge, he put opinion, by which he meant, for example, such insight into ways of action or methods of dealing with men and nature as a statesman might have, but which could not be transmitted to others just because it could not be stated in the form of a reasoned and demonstrated science, and which, just because it was only inner insight, must die with its author. Right opinion might suffice to govern a state and raise it to a glorious position, yet it could not enlighten a people by showing why things must be done in just this way. Then the task of philosophy is to determine the nature of knowledge, and to distinguish between those statements which are knowledge and those which are only opinions. Plato (and Socrates too) interpreted this ideal of knowledge as consisting in deductive demonstration of the sort that is used in mathematics, especially in geometry; he therefore made geometry the propaedeutic to the study of philosophy. While Socrates held that knowledge is deductive demonstration, yet deductive demonstration was not the thing with which he was primarily concerned. His self-appointed task was to re-establish in men's minds the ethical ideals and principles which had been overthrown by the skepticism of the new age and by the sophists. To do that he needed a method by which the grounds for ethical ideals would so harmonize with men's conception of right reasoning that they would accept it of themselves, and this was what he meant by knowledge. Opinion might be correct, but it could not logically bring about its own acceptance, and so it was only at best right opinion, not knowledge. The fact that Socrates found knowledge exemplified in mathematics, and that he probably identified the method of gaining knowledge with mathematical or deductive demonstration, is secondary. Consequently, if we separate the concept of knowledge from any particular method of gaining or justifying knowledge, and define knowledge as those propositions which, if thoroughly understood, together with their proximate and ultimate grounds,[1] will bring about the assent of every unprejudiced mind, because it is justified by a method that experience has always shown to be reliable and never unreliable, we have good historical backing for our definition. The important thing about our definition of knowledge is the notion of its compelling power over unprejudiced people of the widest experience with logical processes. By using the concept of intellectual compulsion upon the intelligent and unprejudiced as the criterion (or *nominal*) definition of knowledge, we are enabled to take an impartial attitude toward the method of gaining such knowledge, if there be such, and to avoid any preconceptions that might later vitiate our whole argument. Yet, at the same time, we can set up an ideal of knowledge that is as high and difficult of attainment as any that has been held in the history of philosophy, one which is in the direct line of philosophical development, inasmuch as it enables us to doubt all preconceptions. Any propositions that cannot meet such a rigid test as this definition of knowledge implies, whether they appear to be correct or not, we shall call "opinion."

It is exactly this sort of knowledge that is required by modern practice at times when it is most important to ascertain the truth, and when chances of failure must be minimized as much as possible. We do not wish to be understood as defending all aspects of jury trial, but the present practice of requiring a unanimous verdict implies ideally that, in order to deprive a man of his life or liberty, the evidence must be so strong that of its own accord it must force any group of twelve unprejudiced men to come to the same conclusion. While it must be admitted that only too rarely a decision is reached on such a basis, yet that fact is due rather to the practical exigencies of the situation than to a failure of the theory. The jury must reach a decision within a short space of time; time for thought and careful investigation, for pondering over all aspects of the case, for testing all preconceptions is not given. Too often there is lacking the intelligence for a thorough understanding of the situation, without which a well-reasoned decision cannot be reached. In science, we can take our time, generations of time if necessary, and we are able to command the best brains of mankind, so we should not be content with a standard of truth or knowledge which is lower than that ideally implied by practical men.

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[1] By "ground" we wish to imply more than a merely deductive ground, and to mean any foundation or basis of any sort for holding a proposition.

§ 4. A proposition which can be said to be knowledge, in the foregoing sense, if there be such, may be said to be "proved" or "demonstrated." The term "proof" has been used very loosely; it has been used to refer, not only to a complete proof or demonstration, but also to any arguments which seem to point even to the "probability" of the correctness of a proposition. In popular speech, to say that a proposition is proved means that the evidence is so strong that anyone ought to accept its truth. While "proof" would thus seem to mean cogent or compelling proof, yet what is called "a proof" of a proposition is something far less than a complete proof. Consequently, we had best take the term "demonstration," not only in its time-honored sense, as referring to a complete proof by means of deduction, but also in the general sense of a cogent or compelling proof, whether deductive or not.

§ 5. We have set our ideal of knowledge as high as we can. There is only one ideal of cogency which is higher, viz., that which cannot be doubted. We may well be suspicious of such a test of truth, when we remember that everything has at some time been doubted, and that there are normal Africans today who cannot comprehend or be convinced that $5+7=12$. The definition of knowledge as the undoubted is otiose, for there is nothing undoubted! If, on the other hand, we define knowledge as that which *normal* men cannot doubt, we have introduced a qualification which can only be circular and lead us into trouble; our definition of "normal" would imply some sort of a logical theory. Or we may succumb to the temptation to define "normal" in such a way as to limit it to those who accept our philosophy! In similar fashion, any definition which makes knowledge that the denial of which implies its reassertion involves a logical theory which must first be investigated before it can be accepted. It cannot, therefore, be taken as a primary definition of what we are seeking, since it unduly narrows our field of investigation.

There is another ideal of knowledge which merits consideration by its popularity—that absolute certainty is found where there is no possibility of error. But it is not possible that there should be any such knowledge—wherever human minds are concerned, error is always possible. Addition is notoriously subject to error; can we say that even a short addition such as $5+7$ is free from any *possibility* of error? We may say that there is practically no possibility of any error being made in that addition, since it can be easily checked; perhaps such a degree of certainty is the highest that can be attained by human minds; but the assertion of its correctness would always be open to the objection that addition is an operation performed by human minds, and any human mind is liable to error, so that we cannot be sure of any addition, not even the simplest. We must therefore free ourselves from the view that knowledge consists in that sort of reasoning in which there is no possibility of error, since such knowledge is strictly impossible for human beings to attain. Hence, we must give up seeking for any such chimerical absolute certainty. The highest practical requirement that can be set is that only those propositions should be admitted to the status of knowledge that compel their acceptance by all unprejudiced and understanding minds who have had a wide experience with different sorts of logical processes and so submit themselves only to those sorts of processes that are beyond logical criticism. This criterion at the same time avoids implying a particular logical theory. Any requirement attempting to be stricter makes knowledge impossible by definition.

§ 6. With this definition of knowledge we have now to face a further problem: Does philosophy deal with knowledge? Or is philosophy merely concerned with opinion?

It is evident that the attitude we take to this problem will determine to a large extent the character of our philosophy. If philosophy deals with knowledge, we must emphasize logic and make it our guide in philosophy; we must seek to criticize, in every possible way, each theory that we meet, subjecting it to the most rigid scrutiny; we must endeavor to discover what are its presuppositions, how secure they are, and how this theory fits in with any others that we have adopted: in short, we can accept nothing which is unable to pass the most severe logical test. Consequently, logic becomes one of the most important of philosophical disciplines. On the other hand, if philosophy deals only with opinions, if it is merely a set of speculations about the world, logic becomes merely a means of confuting our opponents, and can be largely neglected, while the choice of a philosophy will depend mostly upon our individual preference. Indeed, if we are unwilling to submit our own philosophy to logical scrutiny, we ought not to criticize others for their logical shortcomings, and should be tolerant of any and every view which commends itself to any human being. Philosophy would thus become an expression of a vision or mode of feeling the push and drift of life; indeed, it would be quite conceivable that one person might have many philosophies, one for each different mood which may sweep over him. If philosophy is opinion, then there is no one view which can claim the assent of all, and we have no right to expect that anyone else should agree with us, since human beings are so different. We should also be face to face with a difficulty similar to that which the skeptic has to meet when he says that he knows that he knows nothing—for even our opinion that philosophy is only opinion might be only opinion, and we should have nothing to say to those who asserted that philosophy is more than opinion!

§ 7. In defense of the view that philosophy is opinion and not knowledge, it is possible to allege the historic fact that philosophies have corresponded largely to the tempers of those who held them; the greatest philosophers, as well as those who were less original, have worked into their philosophies their own personal temperaments and the characteristics of the age. If such is the fact, does it not then follow that these great historic philosophies have all been only opinions? Then why should we not also choose our own philosophy in accordance with our preferences? If we need comfort in sorrow or help in distress, why not believe in a helpful and comforting God or a future life? Conversely, if we are to choose our philosophy solely because of its practical helpfulness, philosophy becomes a mere matter of opinion, not knowledge. It is rare that pragmatism has been put thus blankly, yet a great deal of its persuasiveness comes from an appeal such as the foregoing; the pragmatist's emphasis that a man's philosophy depends upon his personality undoubtedly tends to carry him to the view that philosophy deals only with opinion. Philosophy and religion are then not far apart, if indeed they are not identified by this conception, except that philosophy does not make as cogent an emotional appeal as most religions. Indeed, if philosophy is really mere opinion, there is little reason that such an emotional argument as this should not be used. How often precisely such an emotional appeal has led men into serious difficulties!

For those who cannot establish the truth of their philosophy by logical reasoning, but are unwilling to make such a direct appeal to emotion and desire, there are other methods of gaining acceptance for their beliefs. One of the most insidious ways, and at the same time a most successful one, of gaining currency for a philosophy is to put it into beautiful literary form. Rare is the man who, like Plato, combines philosophic ability with literary power; when such a man arises, his philosophy is sure to be widely accepted just because of its literary appeal. What could be more attractive than philosophy dressed in poetic figures or shining in scintillating epigram? Even the professional philosopher is attracted by such a beautiful form; if philosophy deals only with opinion, this mode of gaining converts would seem quite legitimate. More than one modern philosophy owes its success to its literary attractiveness. Being literary, it is read; being read with satisfaction, it gains acceptance. If philosophy is opinion, why should not literary beauty and satisfaction be the criterion of a good philosophy? Indeed, if philosophy is for the sake of contemplation, beauty would seem to be essential. Yet how often precisely such persuasive attractiveness has covered self-contradiction or logical weakness! If, on the other hand, philosophy is knowledge, we want the beautiful figures stripped off and the bare bones exposed for rigorous logical scrutiny.

Should neither of these alternatives be acceptable or possible to a philosopher, a third is always open, that of bare assertion. "Trust me, I have the truth," says the dogmatist, and his self-confidence leads others to feel confidence in him and his teachings. Sometimes the dogmatist makes a show of logical strength and at the same time makes his position more persuasive by making only one unproved assertion, or as few as possible, and then following out the consequences of his chosen premises with logical rigor. Such logical rigor is, however, only seeming, for the fundamental premise or premises of the whole philosophical system are not secured, and consequently are likely to be overthrown at any time, with disastrous consequences to the whole edifice. Such a system seems to have logical strength only when we neglect the fact that its fundamental premises are only assumed, not proved. As soon as we see the weakness of its foundations, we recognize that it is ultimately only a dogmatism, and that the whole structure is insecure. Indeed, from one point of view, it might seem that a rigorous following out of the consequences derived from one set of assumed premises would be likely to lead farther from the truth than a dogmatism which made many assumptions. For, if the fundamental set of assumptions deviated, even by only a hair's breadth, from the truth, the farther we carried out their implications, the farther we should be led away from the truth; whereas, if we renounced any attempt to pursue a straight, logical course and allowed ourselves to range at will, making what assumptions we wished and as many as we liked, we might eventually come back to the truth after many wanderings, instead of being constantly led farther and farther away by the very consistency of our logic! If philosophy is opinion, either course may be pursued; no one has the right to criticize or to say that one is more logical than the other—we have entire freedom of thought; no one can object, however extravagant the result we reach. Indeed, there is an attractiveness to the view that philosophy is opinion, because, in that event, we are not bound down to any rules of logic or to any requirements whatsoever; we are at complete liberty; our thoughts may wander as they will, and we may indulge in a riot of speculation without reproach.

Contemporary philosophy seems for the most part to be a curious mixture of dogmatism and skepticism. Some points are violently argued; other points, always the most important ones, are left without demonstration; we are told that the logically more important conceptions are indefinable and that ultimate questions are not susceptible of proof. Philosophy seems both to be and not to be opinion. Yet there have been few attempts to mark out the boundaries of knowledge, and those attempts have largely been dogmatic.

§ 8. The view that philosophy deals only with opinion can never be logically established, for then we should have some knowledge at least—the view that all is opinion must remain only an opinion. Consequently it is evident that if any knowledge can be had, even the least bit of it, we must search for it until we find it; for the least bit of knowledge can put to flight much opinion. It is probable that most skepticism has arisen, not out of dislike for knowledge, but out of despair of gaining any knowledge. Men prefer knowledge and take opinion only when knowledge is not to be had. So we must not despair, but boldly seek for knowledge wherever it is attainable.

Indeed, the self-refuting quality of the position that all is opinion would seem to point to the fact that there must be some knowledge; for the view that all is opinion must always remain merely opinion, doubtful. But if there be knowledge, where is it to be found? Certainly not in philosophy. The agreement of unprejudiced students is not to be found in that field. All contemporary philosophic systems seem, as yet, to be merely opinions. If, however, knowledge is to be found in other fields than philosophy, it behooves philosophy to bestir herself; otherwise she will be found to be salt that hath lost its savor, altogether inferior to genuine knowledge.

It is highly necessary, therefore, to face the task of finding what genuine knowledge there is, and of attempting to analyze the method by which this knowledge is justified as such, in order to discover how more knowledge can be gained. It might be possible thereby to build up more and more knowledge, until we shall be able to do away entirely with the view that philosophy deals only with opinion. In this manner alone can the former queen of the sciences be rescued from the dungheap to which the present confusion in logical theory is slowly but surely consigning her.

CHAPTER II

THE TRILEMMA OF METHOD

§ 1. If now our task is to seek for knowledge, we shall do well to survey the methods of establishing knowledge that have been proposed in the course of the history of human thought. We shall find that there are only three fundamental methods of knowledge, although we shall also need to consider some important subforms. These three are induction, deduction, and intuition.

It is hardly to be hoped that we can discover any new methods as yet unthought of. The history of thought shows us how vain is such a hope. Practically all of the important philosophical ideas and concepts were enunciated in the first few centuries of philosophical speculation. Since the time of the ancients there have been discovered extraordinarily few ideas of real importance. Philosophical forms, like musical forms, seem to be few in number; they were almost all discovered and enunciated by the early geniuses. What progress has come about has been rather by the recombination of old ideas in new configurations, or by the emphasis of ideas that were enunciated but discarded by the ancients, or, most commonly of all, by the discarding of ideas that had been accepted in previous generations. Progress in philosophy has rarely consisted in the discovery of wholly new ideas. After human minds, some of them of the highest genius, have exercised themselves upon philosophic problems for twenty-six centuries and more, upon problems that require not exact investigation and experiment but simply careful thought, what new ideas can we modern thinkers expect to discover? We may rearrange the ideas of the ancients or correct their mistakes, but we can hardly bring forth a single important notion that has never been enunciated before.

Hence we may well give up the attempt to discover any new methods of proof, and recognize that we are limited to the methods which have already been enunciated and used, or to modifications of them. So let us consider these three methods and see how much knowledge we can expect to attain from each of them.

§ 2. Of induction, as historically conceived, little need be said. It has been generally recognized that it only gives probability or opinion, and not knowledge. If the Aristotelian or perfect induction, which proceeds by a complete enumeration of cases, were only possible, we might gain knowledge; but unfortunately it is nearly always inapplicable. Its use is restricted either to the past, where we can know all that has happened, or to human creations, chiefly conceptual subsistents, where the situation can be completely analyzed. Historical or antiquarian knowledge helps us but little in philosophy, whose business is to discover eternal laws; and conceptual analysis has proved similarly vain. Even such a proposition as Jevons', "All the months of the year are of less length than thirty-two days," cannot be proved true unless its applicability is limited to the past; with regard to the future it might become false! The only way to make it eternally true would be to define "month" as a period of less than thirty-two days. Then the proposition would become tautological and lose its significance; its proof could be much more easily deduced than induced; and the possibility of periods of more than thirty-two days would not be eliminated, although they could not be called "months" in the same sense of the word; yet if such periods were actually established, they might, as a matter of fact, be still called by that name! Conceptual analysis does not help greatly in dealing with a non-conceptual world.

Baconian induction has shown itself similarly futile. Perhaps the most searching criticism of it is that this method has never succeeded in discovering or verifying a single fact. Bacon ultimately relies upon "seeing" the generalization in the facts; if the generalization is eventually to be "seen," what guaranty is there that the right one will be seen rather than the wrong one? At most, the Baconian method is an *ars inventiori*, a method of discovery, not a method of proof; it was not constructed to give knowledge but to suggest promising ideas, which must depend for their proof upon other considerations than the Baconian method of constructing tables and seeing in them the needed generalization.

Another method of inductive proof depends upon the analysis of the world into causal relations. If there are causes, and if a cause is what is inevitably and invariably followed by another event, called a result, then it will follow, as an easily deduced consequence, that a cause will always be followed by a result, and that where the result is absent, the cause is always absent. Mill's five laws of induction can be derived from a more careful and complete statement of the implications of this causal principle—laws which were implied by Bacon and Hume and formulated by Mill. Mill's laws are only an explicit formulation of the principle of causality. Consequently, in order to insure their correctness, we need to have this principle of causality itself substantiated—but we find that Mill has failed to establish this principle firmly; it is assumed, not proved. Furthermore, it is now agreed by logicians that Mill, since he demanded in the canon of agreement that *only one* circumstance be varied, and in the canon of difference that *every* circumstance but one be varied, has demanded the impossible; there is no surety that other circumstances, which are unknown or unobserved, do not vary, or that some unknown circumstance remains constant. Since all of Mill's canons are forms of the two principles of agreement and difference, none of these five canons can ever be applied exactly, because it is impossible to know all the circumstances to be found in a concrete situation. Then the only cases in which Mill's canons give us knowledge are those in which the situation can be completely analyzed—the sort of circumstances in which "perfect induction" can be used. Thus Mill's canons either give us no knowledge or else perfect induction could be used better. In this way, Mill's hope of establishing an absolute empiricism has been shown to be illusory.

Similar reasoning would show the uselessness of the so-called "method of exhaustion," in which the proof of a proposition consists in successively denying its opposites. There is little hope of any single individual ever exhausting the possibilities of the universe unless they are first artificially limited by some conceptual means. It has been well remarked that "all various forms of induction resolve themselves into special cases of induction by simple enumeration";[1] hence induction can give only opinion.

§ 3. There is another form of induction, formulated by Whewell from his study of the history of science. He defined induction as "the process of a true Colligation of Facts by means of an exact and appropriate Conception," in which "a new conception is superimposed upon the Facts."[2] While there is much of permanent worth in his account of induction, yet he included so much that is worthless, such as the conception that certain ideas (Kantian categories) are superimposed upon experience, and clothed the whole in such a scholastic and Baconian terminology, that it is necessary to separate his logic from the Kantian metaphysics in which it is clothed and to interpret his involved language into one simpler and clearer. Mill correctly stated Whewell's method as one of hypothesis, ratiocination (deduction), and verification.[1] He criticized it as incapable of giving proof, because it omitted induction as the necessary first step. The correct method, according to Mill, would be the three steps, induction (by which Mill means the application of his canons), ratiocination (meaning deductive inference), and verification: Mill calls this the deductive method;[2] Jevons suggested that it had better be called the combined method.[3] Since that time, this method has been variously styled, often being called the "method of hypothesis." Mill's canons do not belong to this method; for if a generalization is proved true by the use of Mill's canons, what need is there for the other two steps that Mill included in his "deductive method," those of ratiocination and verification? Mill was probably so impressed by the examples of inductive discovery instanced by Whewell that he endeavored to include in his method the steps of ratiocination and verification, which Whewell had shown were actually used in the discovery and verification of scientific laws, although they were logically unnecessary and incompatible with the rest of his theory. Whewell's method, freed from its metaphysical accretions and stated in simple form, is this: that the method of proof of a generalization is by, first, assuming a hypothesis; second, deducing its consequences; and, third, seeing if those deduced consequences are found in experience.

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[1] Richtie, in summing up the work of Keynes and Broad, *Mind*, XXXV, 303.

[2] W. Whewell, *Novum organon renovatum* (1858), chap. v, aph. xiii, xvi.

Contemporary logicians are, however, unanimous in agreeing with Jevons that this inductive method does not give certainty or knowledge, but only probability or opinion. Consequently we take leave of induction, assured by the logicians

[1] J. S. Mill, *A System of Logic* (1904), Bk. III, chap. xiv.

[2] *Ibid.*, Bk. III, chap. xi.

[3] W. S. Jevons, *Elementary Lessons in Logic* (1897), chap. xxx, p. 258.

of our day that by no form of induction, save by the otiose perfect induction, can knowledge be had. As Mr. Russell says, "What is called induction appears to me to be either disguised deduction or a mere method of making plausible guesses."[1]

§ 4. Can, then, knowledge be had by deductive means? Philosophers since Aristotle, and indeed before him, seem to have felt that knowledge can be had in this manner. While the Aristotelian syllogism has been thrown into the wastebasket again and again, only to reappear in the next book of logic, nevertheless the apodictic certainty of deductive demonstration has usually been the ideal of philosophical proof. If there is any certainty anywhere, it would seem to lie in the rigidly concatenated structure of a mathematical or philosophical demonstration. Analysis of the arguments used by the classic philosophers shows that they appealed to deduction to establish their strongest positions. Evidently they had confidence in this method of proof.

Perhaps we can get the characteristics of deduction clearly before our minds by thinking of such a splendid example as *Principia Mathematica.* This is an especially valuable example of deduction because in it a deductive system is analyzed into its elements, and an attempt is made to state all the presuppositions of the system in a simple form.

What, then, are the characteristics of such a deductive system or of any deduction that is carried far enough to analyze out the elements presupposed therein? There is, first, a set of closely concatenated proofs, in which certain propositions are derived from other propositions by combining them in accordance with certain formal principles. Thus, in the demonstration of *2.11, which is the law of excluded middle, the proposition $\vdash : \sim p \vee p \,.\supset .\, p \vee \sim p$ is derived from a previous proposition and combined with the proposition $\vdash . \sim p \vee p$, which had been previously demonstrated, in accordance with *1.11.[1] Thereby the law of excluded middle is demonstrated. The two premises which establish the truth of $\vdash . \sim p \vee p$ are themselves the result of other inferences, and these premises are combined in accordance with a definite principle. In this way both the premises of each piece of inference and the principle of combination form part of the deductive system.

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[1] B. Russell, *The Principles of Mathematics* (1903), p. 11, note.

The same characteristic is found in the ordinary Aristotelian syllogism, where the two premises, "All men are mortal" and "Socrates is a man," are combined in accordance with the *dictum de omni et nullo*, "Whatever is true or denied of the whole is true or denied of the part," to give the conclusion "Socrates is mortal." Or, if we interpret our first premise intensionally to mean that "Humanity implies mortality" and the second to mean that "Socrates has the attribute of mortality," then the *dictum de omni et nullo* would need to be rephrased to mean that "whatever is implied or excluded by an attribute is also implied or excluded by that which has that attribute." Thus the general process of inference in the Aristotelian syllogism is the same as that in logistic or mathematical inference—that of proceeding from premises to a conclusion in accordance with a definite rule of inference.[2]

The second characteristic of a deductive system is a set of primitive or undemonstrated propositions, which form the starting-point of the demonstration. It is evident that in order to demonstrate we must have premises, and, therefore, we must have a first set of premises which cannot themselves be dem-

[1] *1.11 is stated as, "When ϕx can be asserted, where x is a real variable, and $\phi x \supset \psi x$ can be asserted, where x is a real variable, then ψx can be asserted, where x is a real variable," which is the principle of the hypothetical syllogism expressed in symbolic form. ϕx is taken to be $\sim p \vee p$ and ψx is taken to be $p \vee \sim p$; then, because $\vdash . \sim p \vee p$ has been previously established, it follows that $\vdash . p \vee \sim p$ can be asserted.

[2] For a complete proof of this assertion, see chap. vi, §§ 5–18.

onstrated. Every carefully worked out deductive system ought to state all its presuppositions in a form open for careful inspection, including such rules of demonstration as cannot be demonstrated. Since the presuppositions of a system determine its character, it is evident that they are the most important characteristic of the system for the theory of knowledge.

Mr. Russell has also given, as the third and fourth characteristics of a deductive system, a set of definitions, which are not supposed to add any new ideas, being merely convenient abbreviations for more complex expressions, and a set of primitive or undefined ideas or terms which are the building-blocks of the system. When, for example, in *Principia Mathematica*, the idea of equivalence is defined as $p \equiv q \cdot = \cdot p \supset q \cdot q \supset p \; Df$, it is meant that wherever two propositions are asserted to be equivalent, that statement is merely a convenient abbreviation for the more complex statement that each implies the other.[1] These two characteristics are not usually enumerated in a discussion of Aristotelian deduction, because, in using this form of deduction, care is rarely taken to make all its implications plain; the terms used in the deduction are merely assumed, and their meaning is made exact only when ambiguity arises. Careful examination shows, however, that a set of meanings of terms and of primitive ideas is implicitly present in Aristotelian deduction just as much as in mathematical deduction, though not usually expressed.

With this analysis of deduction, we are ready to ask the question, How much knowledge can deduction give? Our analysis has shown that deduction is merely a carefully concatenated chain, so that, in every deductive system, the later propositions are drawn from the earlier propositions—the deduced propositions can only command assent if the premises command assent. As Newton said: "The absolute certainty of a science cannot exceed the certainty of its principles."[1] Thus the whole group of propositions can only give knowledge if its ultimate premises are true. Since the time of Aristotle, this fact has been known, and the ideal of deduction has been valid deduction from true premises. The difficulty has always been to get a set of true primitive propositions. It must, moreover, be remembered that when such a set of primitive propositions has been assumed, deduction cannot guarantee their truth. Deduction, then, is *entirely helpless* by itself to give knowledge. Should we see fit to deny any one of the primitive propositions of a deductive system, we could then refuse assent to all the propositions that are deduced through the use of this one. Deductive necessity or apodictic proof is thus of itself entirely unable to guarantee anything more than merely that its conclusions are correctly derived from the primitive propositions upon which they rest. An apodictic proof can of itself say nothing of the truth of its conclusions; it can only say that the conclusion is necessary, *provided* that the premises and the rule of their combination are accepted. Deductive proof thus shows itself to be merely a strongly welded and unbreakable chain. If we were to climb up a cliff, an unbreakable chain would be a great help; but were such a chain hanging over the cliff, it would be highly important to discover whether its farther end was *securely fastened* before attempting to climb. Any other procedure would court extreme danger.

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[1] It will be shown later (chap xiv) that these last two characteristics, especially the indefinable, are not to be found in all deductive systems, but only in what we shall call pure mathematics. Nevertheless definition, whether it is merely abbreviation or not, and primitive ideas, whether defined or undefined, are found to be implied in all deduction, when its presuppositions are carefully stated. The problems of definition will be treated in chap. vi, § 19.

It is necessary to emphasize the helplessness of deduction to give knowledge, because of the great reliance that philosophers,

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[1] I. Newton, "Letter to Oldenberg," *Opera*, IV, 342, quoted in E. A. Burtt, *Metaphysical Foundations of Physics* (1925), p. 217.

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since the time of Aristotle and Descartes, have placed in deductive demonstration. In the next chapter we shall see how far that reliance went and how pervasive it still is. The reason that deduction is so plausible and that it has been so much used, in spite of its helplessness to give knowledge, is that people rarely attempt to analyze a deductive argument into its ultimate premises, but carry the analysis only two or three steps back; if those two or three steps are correct, the argument is accepted, without seeking any ultimate premises. In this way, deduction comes to have great persuasiveness, although, by itself, it has no cogency at all. Philosophy is the deliberate effort to seek the ultimate premises of every argument and to evaluate them; so philosophy must give up purely deductive argument, if it is to attain knowledge. Apodictic reasoning or necessary deduction shows itself, for the purposes of the theory of knowledge, the very weakest of all forms of reasoning, inasmuch as it cannot establish its own first premises. The foundations of knowledge, at least, must be nondeductive.

§ 5. As a consequence of this inherent failure of deduction to furnish us with knowledge, there was invented that very plausible way to knowledge, the coherence theory. If only the chain of deductions could be made to return upon itself,[1] the problem of finding the first premise would be solved. Truth, for this theory, consists in the coherence of a particular proposition with a system of propositions. The advocates of this criterion of truth have been able to adduce various persuasive illustrations of the use of this principle, such as Harvey's discovery of the circulation of the blood.[2] It must be admitted that coherence seems to play a part in the validation of scientific laws, but whether it is in itself a sufficient criterion is a different question. For example, Aristotle's physics formed a system that was coherent in itself. Shall we say, then, that the truth of the proposition, some things tend naturally upward, such as fire, and others naturally downward, such as stones, depends upon the fact that it coheres with Aristotle's physics? Certainly Galileo did not think so!

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[1] B. Bosanquet, *Logic* (1888), II, 222.

[2] B. Bosanquet, *Implication and Linear Inference* (1920), pp. 75 ff. Cf. also chap. vii, § 19 of this book.

The logical difficulty in applying the criterion of coherence is that, in order to test the truth of any proposition in accordance with this theory, the system to which it belongs must first be known. That is, coherence presupposes the existence of a body of truth. It asserts that any proposition which fits in with what is true is itself true, while what does not fit in with the truth is false. But how do we justify our first set of truths? That is exactly the thing for which we are searching—to validate the first truths, which are presupposed by the coherence theory.

There are two possible answers that the coherence theory can give to this question as to the way by which to come by first truths. One answer is that when we accumulate any large number of propositions which fit in with each other, we have attained truth. This is the usual meaning of the coherence theory,[1] and at first sight appears to be a workable interpretation of how to find truth. Amass a group of coherent propositions, the larger the better, and you have truth or knowledge. But we have had too bitter experience with coherent systems in the past, such as the Ptolemaic astronomy or the phlogiston theory of heat, to accept such a criterion of truth. On this basis, a good novelist or a clever liar would be as much a purveyor of truth as the most painstaking scientist or historian. As long as his statements hung together, we would have to believe him! The insane victim of delusions has created a system of ideas, and all other statements are true or false according to whether they do or do not cohere with his system of ideas. Tell the man who thinks that he is Julius Caesar or emperor of the world that such a statement cannot be true because he is confined in the asylum, and he will give a quite plausible reply, such as the explanation that the jealousy of some pretender to the throne has led to his confinement, or some equally coherent story. To the insane man, coherence is the test of truth, and any statement that does not cohere with his ideas is rejected as false. Mere coherence in a system or agreement with what we already believe cannot be a sufficient test of truth, unless we are willing to validate the reasoning of the insane and that supporting many outworn popular prejudices.

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[1] Cf. the statement of R. H. Dotterer, in *Beginner's Logic*, p. 333.

The reply is made by the advocates of the coherence theory that the Aristotelian physics or the phlogiston theory of heat was rejected exactly because it did not cohere—facts were found which did not fit in with these theories. These facts formed a still larger and more inclusive system, which itself included all the facts that the Aristotelian physics or the phlogiston theory had subsumed and more yet. Then the test for the truth of a system is not merely that it is a system, but whether it can cohere with a still larger system. Aristotelian physics is false because it does not cohere with Newtonian physics, and Newtonian physics may be false because it fails to cohere with certain phenomena which are explained by the Einsteinian physics, etc. The larger system itself is only true because it is subsumed in a still larger whole, and the only ultimate criterion of truth is the universe as a whole. This is, indeed, the view of the more consistent and far-sighted advocates of the coherence theory. It is the other possible answer to the problem of justifying first truths. As Bosanquet writes, "Nothing is really certain except the whole, for it is impossible to say that, apart from conditions which the whole furnishes, anything would be what it is."[1] In other words, the whole must be known in order to render the operation of the parts intelligible.[2] Thus, in order to understand and validate the operations of a sheriff, the whole British constitution must be understood; in order to understand the British constitution, the whole which it again is a part of, that is, the general laws of political action and organization, must be understood; in order to understand these general laws, the political constitution of mankind must be understood; and in order to understand that constitution of mankind, the whole nature of the universe must be understood. Hence we must know the nature of the whole universe before we can determine accurately the function of even a minor officer of the law. Likewise the truth of Harvey's doctrine of the circulation of the blood could not be known until the whole of the physiology of the human body is understood, and that could not be understood until the whole of the universe is understood.

It is thus seen how futile is the hope that the coherence theory can help us to be sure of any knowledge whatsoever, when that theory is stated in a form free from obvious objections. Its plausibility lies in the fact that we can break up the universe into parts or systems *after* they have been discovered and use these systems for the purpose of drawing inferences concerning other parts of these systems. Certainly no truth was ever discovered or validated because of its coherence with the whole of the universe, not even the coherence theory itself! Absolutistic coherence cannot validate any system. Bradley was merely drawing out one of the implications of the coherence theory of truth when he asserted that the only truth is the unknowable Absolute itself.

The coherence theory means either that truth is what coheres with what I already believe—in which case it is an appeal to prejudice; or it means that any set of propositions which hang together are true—in which case it rationalizes the reasoning of the insane man and the clever liar; or, in its absolutistic form, it means that we must know everything before we can know anything. Therefore we must take leave of coherence as a possible guide to knowledge and seek another.

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[1] *Implication and Linear Inference*, p. 68.

[2] *Ibid.*, p. 71.

§ 6. The failure of deduction to produce knowledge, which was realized as early as Aristotle, led philosophers to seek some other method of establishing the fundamental propositions necessary for deductive inference. It has been argued that "if there is any knowledge of general truths at all, there must be some knowledge of general truths which is independent of empirical evidence, i.e., does not depend upon the data of sense."[1] If any deductive system at all is to be certain, there must be some non-deductive method of establishing as certain the postulates which are the foundations of that system.[2]

The usual way in which these first premises are supposed to be established is by some form of what we may call direct knowledge or intuition. Strictly speaking, intuition refers only to the mystical insight; but there are a number of allied ways to truth, which, under criticism, resolve themselves into appeals to intuition or deduction, and so had best be considered at this point. They are: the a priori truth, the necessary truth, and the self-evident truth. These terms are often interchanged in meaning; we analyze three typical appeals to cogency under these three names, while recognizing the fluidity in the meaning of these words.

The Kantian a priori truths, as we shall see in the next chapter,[1] are not intuitions, but deductions from a certain constitution of the mind. We shall see that Kant asserts a certain necessary constitution of the mind, from which it follows that the forms of the sensibility and the categories of the understanding must be a priori. Kant's critical philosophy endeavored to make what are usually considered to be the fundamental premises for deduction, such as the axiom of causality, *deductions* from something further. Thereby, of course, he merely pushed the problem one step farther back and had to face the problem of establishing his primal premises in some other way. A priori propositions, in Kant's sense, are deduced propositions.

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[1] B. Russell, *Our Knowledge of the External World as a Field for Scientific Method in Philosophy* (1915), p. 56.

[2] Whether these postulates must necessarily be independent of empirical evidence is a quite separate question, which we must leave until a later stage of our investigation.

The concept of the a priori truth has also been used, since Kant, in a different sense—to refer to those truths which are necessary in order to make certain accepted deductions, but which cannot be derived from experience. It is said that

> Only those ultimate logical invariants can be called apriori, which lie at the basis of any determination of a connection according to natural law. A cognition is called apriori, not in any sense as if it were prior to experience, but because and in so far as it is contained as a necessary premise in every *valid judgment* concerning necessary facts.[2]

It is thus necessary, before we can be sure of any a priori truths, to have *valid judgments*, in order that we can establish the truth of these a priori truths by showing their necessity for the purpose of establishing the truth of our valid judgments. Truth must be had before the foundations of truth, the a priori truths, can be laid! The argument is therefore completely circular. In similar fashion it is argued, for instance, that we have accepted as true the laws of finite numbers, and find that finite numbers involve the principle of mathematical induction; consequently mathematical induction is called an a priori truth, because it is necessary in order to validate the characteristics of finite numbers. Then the question arises, How do we know that our statement of the characteristics of finite numbers is correct? It is mathematically correct because it is a correct deduction from the fundamental postulates that the laws of numbers are derived from—and one of these fundamental postulates, in the case of finite numbers, is mathematical induction! Thus the argument is again a complete circle. In this sense, the a priori truth is validated because it is implied in what we call "natural law" or "fact," i.e., truth, and so it is either a *petitio* or else it cannot be ultimate. The a priori method must first assume the existence of truth or "valid judgment" in order to validate any truth!

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[1] Chap. iii, § 19.

[2] E. Cassirer, *Substance and Function and Einstein's Theory of Relativity* (1923), p. 269. Italics mine.

In other cases, the term, a priori, upon careful analysis, will be found to be merely another name for the intuitive truth or a covert appeal to deduction or some other of the ways of reaching certainty which we here analyze.

§ 7. The concept of the necessary truth—the statement that is so because it cannot be otherwise—also conceals another covert appeal to some one of these two fundamental methods of gaining truth, deduction or intuition. Take, for example, such a proposition as "$2+2=4$" or "The sum of the angles of a triangle is equal to two right angles." Wherein does its necessity consist? Unless necessity is a meaningless word, it must be explicable. Then $2+2=4$ because it is a necessary conclusion from some premises. Given the definition of 2, of 4, of the method of their combination, addition, of equality, and of all that is presupposed by those ideas, then it can be shown that $2+2$ must be 4 and can be nothing else—that is, the necessity of this proposition is purely deductive. The necessity of a geometrical proposition such as that about the angles of a triangle also consists in the fact that it is implied by the primitive propositions of its system. Practically all so-called "necessary truths" can be shown to be necessary only because they are implied by the primitive postulates of a certain system which is itself accepted. That this is the case can easily be shown by the experiment of denying one of the presuppositions of that system, whereupon these "necessary truths" cease to be necessary. For example, the proposition, "A square is round is absurd" is necessary because of the system of meanings implied in the definitions of "square," "round," "absurd," the copula, the nature of geometrical space, and the law of non-contradiction; so that the proposition "A square is round" violates the law of non-contradiction. Without making all the assumptions implied by these definitions and principles (explicitly or tacitly), such a statement would not be necessary at all, as would be shown by the experiment of asserting that geometrical space is flexible, so that the lines of a "square" could be distorted into circular shape. Such a statement as "If Socrates is a man and all men are mortal, then Socrates is mortal" is similar to these others in its logical characteristics. Necessary proofs of this sort are purely deductive and depend upon the assumptions underlying the deduction. In this case, if we deny the principle of the categorical syllogism, which is one of the presuppositions of this statement, the conclusion no longer holds. These necessary truths are apodictic and share the weakness of all apodictic truths—their truth depends on the truth of the premises from which they are deduced; consequently they are no more and no less true than the first premises which imply them. Similarly, the example of a necessary proposition given by Mr. Lewis, "At least one of the two, p or not-p is true,"[1] is necessary because it is a corollary of proposition 3.33 in his system of strict implication. Without that system or its equivalent, it would not be necessary any more than the primitive propositions of the system are necessary. The reason that the assertion of necessary truths is so plausible is that, as practical men, we do not stop to carry the proof of a proposition back to its ultimate sources, but only carry it back a few steps—we see that a given proposition is required by some others we have already accepted, and call it "necessary"! But, as philosophers, we cannot be content with the limited and convenient procedure of practical life; we must carry our criticism through to the end.

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[1] C. I. Lewis, *A Survey of Symbolic Logic* (1918), p. 337.

The common rationalistic view that a necessary proposition is one whose denial implies its reassertion[1] is similar to the foregoing. Such a statement can mean only that, taking for granted the ideas of implication and the processes of inference, together with the law of non-contradiction, and very likely a great deal more of deductive logic, including a few important primitive propositions, we can show that the denial of this "necessary proposition" implies its affirmation. But if we do not first assume at least the method of deductive inference or the law of non-contradiction, we can show nothing of the sort, as will be shown by the experiment of denying any one of these important presuppositions—here, too, the appeal to a necessary truth is an appeal to a deductive system which is assumed. Either such an argument is a *petitio principii* or else it is an appeal to deduction.

There is just one exception, only one truth which may be said to imply its affirmation—here, however, the implication is not a matter of reasoning, but purely one of experience—the proposition that the denial of thinking or experiencing is itself a process of thinking or experiencing. Outside of this one truth there has never been nor can be discovered another such truth, the *experience* of whose denial is an *experience* of its affirmation. This proposition, which is the truth underlying the

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[1] *Ibid.*, p. 338; J. Royce, *William James and Other Essays* (1911), p. 251.

cogito ergo sum, is therefore different in kind from all the other necessary truths that have been propounded.[1]

It might be thought that there are certain general propositions which "apply to all things and all properties,"[2] and so may be considered certain or true. How can we tell that they "apply to all things and all properties"? By experience? By trying one thing after another? Horrors! That would make such a general proposition an empirical truth and rob it of its certainty. How else can we know that it applies always and to everything? Only if it is made true by definition. Mr. Russell gives the following as an example of this sort of necessary truth: "If anything has a certain property, and whatever has this property has a certain other property, then the thing in question has the other property."[3] At first sight this proposition does seem universally and generally true, but upon analysis it is found to assume its own truth. It assumes that "whatever has this property has a certain other property." If that part of the proposition is *assumed* to be true, and if the rules of deduction, especially the rule of the hypothetical syllogism (which Mr. Russell has found necessary to state as an independent primitive proposition in *Principia Mathematica*, *1.11), are assumed to be true, then it follows as a matter of deduction that the thing which has a certain property will also have the other property. But, if we should deny the principle of the hypothetical syllogism, Mr. Russell's self-evident and universally true proposition would no longer be true. It is thus found to be merely the statement of the ordinarily accepted deductive procedure put in a slightly different form. Consequently it is true only if we first assume the procedure of deduction—it assumes its own truth in assuming the deductive procedure. Such propositions as this one will not help us in the search for the ultimate justification of knowledge.

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[1] Cf. chap. xi, § 4.

[2] Russell, *Scientific Method*, p. 57.

[3] *Ibid.*

Universal and necessary propositions of this sort are only true on condition that the assumptions which underlie them are accepted, and that the deductive reasoning which establishes them as "necessary" is accepted. If we should impugn either the assumptions which underlie the deductive system assumed or the methods of deduction used to infer them, these propositions would be no longer universal or necessary. Hence they have only a relative universality and necessity—relative to the original assumptions of the system to which they belong —which is a characteristic of all purely deductive truths. For example, $5+7=12$ is a universally and necessarily true proposition only on condition that we are dealing with a decimal system in which the ordinary rules of arithmetic are applicable. If we are dealing with a duodecimal system, $5+7=10$ would be just as true.[1] Hence, the truth of $5+7=12$ is universal only because it is relative to the assumptions underlying the system. Such relative necessity and universality is not the necessity and universality required for the establishment of knowledge.

It has been commonly asserted that certain logical principles are assumed in all reasoning; for the purpose of examining that viewpoint we take one of the most general of all principles, the law of non-contradiction, and show how its application is limited.[2] A simple statement of this principle is, "Denial and affirmation of the self-same judgment is wholly inadmissible,"[1] or "Nothing can both have a certain quality and not have it."[2] Such a statement of the law would rule out the view that two opposite forces contradict each other—but is not the law stated as it is precisely because it is desired to avoid just such apparent absurdities? After all, two opposing forces do oppose each other in a way similar to the fashion in which two contradictory propositions oppose each other, but experience has shown that the resultant of two equal and opposite statements is the falsehood of one or both, while the resultant of two equal and opposite forces is rest; so that, because of this *experience* of the different way in which they behave, opposing forces are not usually called contradictory.

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[1] If we take 10 to mean the unit of next higher order, we have an exact parallel in the English monetary system, where $5d.+7d.=1/0$, not $1/2$.

[2] Messrs. Russell and Whitehead (in *Principia Mathematica*) have stated that, in spite of the fame of this law, they have found few occasions for its use. It is, however, implied in the primitive idea of negation and the definition of implication in *1.01; otherwise it would not be true that "when 'p implies q' holds, then either p is false or q is true; hence if p is true, q must be true"—the inference that, if p is true, q must be true is made only by the use of this law of non-contradiction!

A closer approximation to the denial of the law of non-contradiction is found in the common experience of holding two contradictory attitudes at the same time. Who has not seen that a young lady not only can say "Yes" and "No" at the same time, but that she delights in doing so? Desiring and hating the same thing simultaneously is a common experience; if it is objected that desiring and hating are contraries, not contradictories, it is also true that the experience of desiring and not desiring the same thing at the same time is common. Most philosophers have succeeded in being both logical and illogical! The law of non-contradiction does not apply to psychological phenomena just as it does not apply to physical phenomena, and, for the same reason, *experience* shows that its use would lead to confusion. This difference between logic and psychology should have warned those philosophers who insist that logic is psychological in its nature. The law of non-contradiction applies only in the realm of concepts.

Other exceptions to the law of non-contradiction are such statements as: a line can be both straight and non-straight at the same time—straight to the eye and irregular under the microscope; or, the same object may be moving slowly and quickly, or, it may be both large and small, depending on the standard of reference. According to our new physics, a body may be both at rest (with reference to certain axes of reference) and moving (with reference to other axes) at the same time, although motion and rest are contradictories.

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[1] F. H. Bradley, *The Principles of Logic* (1883), p. 137.

[2] B. Russell, *The Problems of Philosophy*, p. 177.

Hegel's denial of the law of non-contradiction is well known. In attacking the law of excluded middle, he showed that it is not true that there is no alternative between *A* and non-*A*, but that *A* itself "is either $+A$ or $-A$, and also both $+A$ and $-A$."[1] Thus "man," as meaning a human being, contains both "German" and "not-German"; "color" is both "red" and "not-red," etc.

In reply to the foregoing, it would probably be urged that these statements are not contradictions of the law of non-contradiction, because the same thing cannot be both true and false *in the same respect*, and we have been making statements which are true in different respects. But what is meant by "different respects"? When a situation is analyzed so that the same predicate will not be used in "different respects," is it not the case that an attempt is made to state the situation in such a manner that it will be possible to deal with it according to the law of non-contradiction, and is not the law of non-contradiction exactly the criterion of whether the "respects" are the same or different? In like manner, Hegel's argument would be refuted by asserting that the law of non-contradiction does not mean what he takes it to mean, instead it asserts that "the same attribute (or predicate) cannot at the same time belong and not belong to the same subject in the same respect."[2]

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[1] *Wissenschaft der Logik*, Bk. II, Absch. I, Kap. 2, C, Anmerk. 2. Cf. also Baillie, *Hegel's Logic*, pp. 297–300.

[2] Aristotle *Metaphysics* 1005*b*.

Thus the applicability of the law of non-contradiction is maintained by applying it first to see whether it is applicable, then rejecting those cases in which it is not applicable! In this manner the "universality" of the law is maintained. Since this law cannot be denied or omitted without permitting deductions which are usually, though not always, contrary to experience, the law is assumed in most of our abstract deductive systems for purposes of deduction. The law applies to concrete things only when it does—only when things can be analyzed in such a way that the law can apply. In other cases we save the situation by saying that "the law does not apply."

The experiential source of the law of non-contradiction would seem to be, not the having of desires, but the satisfaction of desires. Two contradictory desires may be entertained at the same time, but they cannot be satisfied at the same time; since, for most purposes, satisfaction or attainment of a goal is more important than the setting up of a goal, this non-contradiction, found in the experience of satisfaction, is usually taken as a law of thought. It holds true, consequently, only of those experiences which conform to the law.

In a similar manner these "principles which are assumed in all reasoning" are limited in each case by the nature of the material with which we are concerned. As in the case of the law of non-contradiction, they are dispensed with when the material is inappropriate to them. These truths are true only in situations in which they apply—that is, they are true only in situations in which they are true. Their indefeasible universality is saved by declaring that they do not apply to situations in which, if they were applied, a false conclusion would result.

Even tautologous propositions, such as "*A* is *A*," "*B* is *B*," "I shall be what I shall be," "Nothing in verse as in prose, is to be nothing,"[1] cannot be accepted as necessary or intuitive truths. Ever since Fichte, by means of a "transcendental deduction" showed that the truth of "*A* is *A*" implies the Absolute Ego,[1] philosophers should have been cautious about admitting even "*A* is *A*" as an absolutely certain truth. Fichte's argument is that if "*A* is *A*" is to be an eternally necessary truth, it can only be because there is something which eternally keeps *A the same with itself*, i.e., there must be an absolute Subject which remains eternally the same; otherwise *A* and *A* may not remain the one the same as the other, and "*A* is *A*" might become false.[2] The nerve of this argument lies in the fact that "*A* is *A*" is only true when the first *A* and the second *A* are *the same;* i.e., in asserting that "*A* is *A*" is eternally true, we are asserting that *A* remains eternally the same with itself. Thus the truth of "*A* is *A*" is not absolute; it depends upon the further assumption that *A* remains unchanged in passing from subject to predicate. Geometricians, in attacking the validity of the method of proving the equality of angles, etc., by superimposition, have shown how faulty sometimes is the assertion that "this angle is the same as itself" when there is a change in position. *A* and *A* may not remain identical! Of course, in dealing with a supposedly eternal and unchanging world of truth, such as the Leibnitzian intelligible world, the proposition "*A* is *A*" must remain eternally true, just because that world remains unchanged; but the converse argument that the truth of "*A* is *A*" proves the existence of an unchanging world is circular, in that it assumes an unchanging world in assuming the truth of "*A* is *A*"! While we do not deny that there may be a world of unchanging truths, yet it is sheer dogmatism to assert, without examination, the truth of any identity, even "*A* is *A*." If we cannot accept even identical statements as necessarily true, how much less can we accept unquestioningly any other supposed universal and necessary truths.

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[1] Leibnitz, *New Essays*, Bk. IV, chap. ii, sec. 1 (p. 405 of A. G. Langley's translation) gives a list of these.

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[1] *Grundlage*, Part I, sec. 1.

[2] The validity of this "transcendental deduction" will have to be judged in accordance with an analysis of the method of "transcendental deduction," which will be taken up in the next chapter in connection with the discussion of Kant. Cf. chap. iii, § 19.

In other uses of the term, necessary truth, appeal is made to self-evidence or intuition. It is held that a truth is "necessary" because it evidences itself. All so-called "necessary truths" can be analyzed into covert appeals to deduction from an already assumed set of principles or appeals to some form of self-evidence or mystical insight.

§ 8. Before we take up the matter of self-evidence, it is well, in order to complete our survey of ultimate methods of justifying knowledge, to consider a closely allied method of establishing first principles, namely, the appeal to authority. In practical matters we often appeal to authority with advantageous results. But the philosopher can never tolerate an appeal to authority, for he seeks to go to the root of his problem; consequently the question always arises, Whence does this authority get his authority? and, Is it really an authority? Even if the testing of an authority is by an appeal to another authority, that again must be tested; the result is a regress which can only end by resorting to some other method of establishing the truth—usually by an appeal to intuition. The appeal to authority is always unphilosophical.

§ 9. A self-evident truth is one that establishes its own self. In distinction to deduction, a priori truths, or mystic intuition, self-evidence is the characteristic of a proposition which claims truth of itself without the aid of anything outside of itself.

It is not to be denied that there are propositions which appeal to individuals as of themselves self-evident or conclusively persuasive. Descartes discovered many of them; both since and before his time others have also asserted that they have discovered many more. These self-evident propositions need, however, to be carefully examined—what was self-evident to Descartes has not always been self-evident to others; and some of Descartes's self-evident truths have come to be almost universally denied self-evidence. Different individuals disagree greatly as to just what propositions are self-evident—how can we decide between them? Self-evidence, because of its own nature, is not amenable to criticism—either a proposition is or else it is not self-evident; the very fact that it is self-evident means that there is no other reason for its being self-evident than just that it is so. Hence, there is no way to decide between conflicting claims to self-evidence, except by appealing to some other standard of truth than self-evidence.

There is, in addition, the fact that some self-evident propositions have been found to be false or to have lost their claim to be self-evident. To Descartes, the principle of causality seemed self-evident; yet Hume, by a more careful analysis, impugned it so severely that to careful thinkers it has ceased to be self-evident. In mathematics, the parallel axiom has been shown to be neither necessary nor self-evident. Poincaré gives a list of apparently self-evident mathematical truths which have been shown to be false,[1] including an axiom supposed self-evident in the time of Kant, but since shown false, that "every continuous function can be differentiated."[2] Mr. Russell gives, as an example of a statement which appears self-evident, but which, when assumed, leads to contradictions, the proposition that a part cannot be mathematically similar (have the same number of parts) to its whole.[3] In the theory of probability, James Bernouilli candidly records two false solutions of a problem which he at first thought to be self-evident.[1] Many other cases of supposed self-evident truths could be given: Joule felt that the law of conservation of energy is self-evident[2]—a law which to the naïve mind seems more often broken than obeyed; Spencer found it "impossible to imagine a consciousness so constituted as not to give" the law of the persistence of force.[3] As Whewell remarks of the earlier truths of science, "From being asserted at first as strange discoveries, such truths come at last to be implied as almost self-evident axioms."[4] The many mistakes that have been made in the self-evidence of propositions show how necessary it is to criticize their self-evidence; yet, as we have seen, self-evidence, from its very nature, is uncriticizable.

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[1] H. Poincaré, *The Value of Science*, chap. i, sec. ii.

[2] *Ibid.*, p. 458, note.

[3] *Principles of Mathematics*, p. 360. An exact statement of the axiom referred to is, "If one class be wholly contained in, but not co-extensive with, another, then the one has not the same number of terms as the other" (*ibid.*, p. 371).

In fact, self-evidence is merely a way of saying, this proposition is true, but I do not know how it is true—it is a method of avoiding intellectual inquiry. Hence, it is thoroughly unphilosophical, because philosophy, by its very nature, attempts to criticize its own foundations. Ever since the experience of eighteenth-century philosophers with Descartes' self-evident truths, we should have been wary of such an *ignis fatuus*, and should have seen that the claim of self-evidence *always* needs a criticism that it cannot give to itself. It is hard to avoid the suspicion that self-evidence means merely a great degree of familiarity with, and wide use of, a proposition, or congruence with the prejudices and opinions of an individual, so that it is a purely psychological phenomenon which has no logical significance. The fact that the modern supporters of self-evidence admit that it has degrees[5] shows that it is a psychological, not a logical, phenomenon; for if it were a purely logical phenomenon, no degrees would be expected—a proposition would either be self-evident or not, just as a concrete statement is either true or false; but if self-evidence is a psychological experience, we should expect it to have degrees, just as other emotional experiences have degrees. Self-evidence is not an experience to which we can attribute any logical, evidential value.

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[1] W. S. Jevons, *The Principles of Science* (1892), p. 213.

[2] J. Ward, *Naturalism and Agnosticism* (1899), Vol. I, p. 173.

[3] *Ibid.*, I, 215.

[4] W. Whewell, *History of the Inductive Sciences* (1901, 1846), I, 45.

[5] B. Russell, *The Problems of Philosophy*, p. 184.

§ 10. Now that we have made clear some of the misconceptions implied by the a priori, by universal and necessary truths, and by self-evidence, we can attempt to evaluate the third of the great methods of gaining knowledge, namely, intuition in the proper sense of the word, or mystical insight. It is undoubtedly true that intuition does give knowledge. In fact, practically all important discoveries come as intuitions. Not simply Sir William Hamilton's discovery of quaternions, but even such simple things as the solution of a puzzle often come as an intuition—a sudden insight into the correct solution. Poincaré has left us an account of how some of his mathematical discoveries came to him—after months of working over a problem, seemingly without result, he gave it up as hopeless; then later the solution flashed upon him, although afterward it took him months to work out the full demonstration of that solution.[1] Mozart heard his symphony all at once; Goethe held that all poetry is inspired. This mystical experience of discovery is an experience that comes to every seeker after knowledge and probably to every man sooner or later.

Furthermore, it is possible for the mystic intuition to validate itself as a method. The deductive method can never prove its truth as a method of proof, for it must always assume, not only its premises, but also its principles of combination. The mystic intuition may include a revelation, not only of positive truth, but also of its own validity as a method of gaining truth.

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[1] H. Poincaré, *Science and Method*, Bk. I, chap. iii.

While few mystics have had such a revelation, that is probably because they have not been concerned with the problem of method, merely with the problem of finding the truth. Nevertheless the mystic intuition does often bring with it an immense certainty, so that truth thus revealed may seem more certain than any other truth—far more certain than any merely syllogistically demonstrated statement. Yet such has not always been the case, for the great mystics have usually recognized that Satan can give revelations, just as may the Holy Spirit, so they have tested their revelations by their congruity or lack of congruity with the teachings of the church. They thus relied upon a different method than intuition for the purpose of testing their revelations. Nevertheless, to many persons, intuition brings its own certainty with it, and intuitive revelations become unshakable truths.

But, alas, this certainty gained by the method of intuition is logically confined to the mystic himself. If I accept a revelation that has come to another, it is because I take him as a worthy authority; consequently, there is needed some test for the trustworthiness of his authority. That test is usually deductive, i.e., I assume that a person possessing certain characteristics is worthy of belief, and, finding that he possesses these characteristics, I accept his authority. Thus another method than intuition is ultimately relied upon to justify my belief. The intuitive revelation, no matter how convincing it may be to the person who has it, cannot of itself convince another. It is purely personal and private; and the moment the mystic attempts to convince another person that his intuition is correct, he is appealing to another method of proof than the intuitive method. Intuition is *wholly* subjective. It may prove itself to the person who has it, but it cannot prove itself to another person.

It may be thought that intuition could give certainty, inasmuch as all who underwent certain conditions would have the same intuition, just as all who see a chair experience it as a chair. Unfortunately such has not been the case. Only the man who has spent considerable time studying music and has shown aptitude for music gets musical revelations; only the person who has studied poetry and methods of writing it and has shown poetic ability gets poetic inspirations, and so in other lines of activity. Furthermore, the certainty of the mystic lasts just as long as he has no doubts. Once doubt arises as to the genuineness of his revelation, it cannot be allayed by the method of intuition; it must be relieved, if at all, by appealing to some other standard of proof. As long as illusions, hallucinations, and self-deceptions are found in experience, certainty cannot be attained by the mere acceptance of what is strongly impressed upon one. As a matter of fact, intuitions have been strongly discordant. The Christian mystic has had revelations of the Christian God, and the Hindu of the Hindu Brahm; unless we wish to commit intellectual suicide by accepting as true all of the discordant deliverances of "geniuses, ladies, and lunatics,"[1] we must refuse to accept intuition as in any sense a *guaranty* of knowledge. Moreover, the mystical intuition, in addition to its own subjectivity, is also guilty of all the faults of the self-evident truth. Intuition may sometimes give knowledge, but it cannot guarantee knowledge.

§ 11. We have thus considered the various methods of justifying knowledge and have found that the fundamentally important ones are three—induction, deduction, and intuition. Other subforms have also been considered, but none of them have been found to give knowledge. Of these three chief ways to knowledge, we have found induction condemned by all logicians as failing to produce more than probability; deduction showed itself the weakest of all, in that it is merely a method of connecting, in accordance with certain assumed principles, a conclusion with premises, and that it can neither prove its first premises nor its correctness as a method of gaining knowledge; while intuition showed itself, at most, to be convincing only to the individual who has it, and not always to him. The history of thought has shown us how futile it is to expect to discover any new method of gaining knowledge; we are limited to these three. In this trilemma, where shall we turn? Induction is said to be only probable; the metaphysical certainty of the apodictic reasoning of deductive necessity shows itself to be without certain foundation; intuition shows itself a vain hope. Shall we say that there is no certainty or truth to be had, with the consequence that all mathematics becomes an uncertain construction upon an unstable foundation, all science only an easily overturned guess, all philosophy a vain speculation, all practical activity a futile experiment in an unknowable world? Shall we conclude that knowledge is not to be had and agreement between men on any subject whatever is not to be obtained without dogmatism? Before we give up to such a terrible skepticism, which would destroy philosophy as well as science and mathematics, we must see whether there is any escape from the trilemma. Deduction and intuition we have ourselves examined; is it possible that we have made a mistake in taking the word of contemporary logicians as to the uncertainty of inductive proof, and that a better conception of induction will show us how to gain certainty by its means? Before we consider that possibility, we may gain some light upon our problem by making a brief historical survey of the methods of proof used by the great philosophers and scientists.

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[1] F. C. S. Schiller, *Formal Logic* (1912), p. 237.

CHAPTER III

THE SEARCH FOR A METHOD IN PHILOSOPHY

§ 1. The problem of method in philosophy was not the first philosophical problem to arise. Man starts to think by whatever method he is accustomed to use, and only stops to reflect upon his method of thinking when he finds that his thought has led him into blind alleys or self-contradictions. It was when the sophists of Athens had showed its thinkers that clever arguments could seemingly prove the most diverse and impossible statements, and when Parmenides had established paradoxical conclusions, that men first set out definitely to delimit and define the characteristics of demonstration as distinguished from fallacious argument.

In seeking to analyze the method of proof used by philosophers, we must distinguish between the method of proof as conceived by these thinkers themselves and that method which they themselves actually did use, i.e., the means whereby they succeeded in impressing other people with the truth of their philosophies. Frequently we shall find that these were two different methods; even in the case of great scientists, we shall find quite opposite methods in theory and in practice. The history of philosophic method has not yet been written, nor can that be done here. We can only give a few brief impressions of the course of philosophic thought.

§ 2. Philosophy began with speculation as a consequence of man's wonder about the world. For the first philosophers there was no problem of method. They were concerned with the solution of physical and metaphysical problems; the very sophisticated process of reflecting upon reflection could not come until later. Even though we had their works in full, we should not find any discussion of method. But from our knowledge of human psychology, we can determine what was the method which they must have used, since they could only use the method of reflection commonly employed by unsophisticated men.

It is a fact many times pointed out, perhaps most convincingly by Mr. Dewey,[1] that the method of solving a problem among unsophisticated thinkers is to assume a hypothesis and then to see how well the facts of experience correspond with the necessary consequences of that hypothesis. By an analysis of a person's thought in making an engagement, in puzzling out the use of a queer-looking pole on a ferryboat, or the peculiar behavior of soapsuds bubbles on the mouth of a warm, freshly washed tumbler, Mr. Dewey showed that in each case the solution is attained by the method of making various hypotheses and seeing which one enables the making of deductions that fit the circumstances of the case.

This method was very likely the one used by these early thinkers. All things are made of water, said Thales; and the plausibility of this hypothesis consists in the various forms that water assumes. In all probability it was the failure of water to take on all the characteristics of the opposites found in things that induced his successor, Anixamander, to assume a boundless something as the source of these opposites. It was again the failure of these two philosophers to explain how the variety of phenomena could be produced—their inability to give a causal explanation—that induced Anaximenes to resort to the hypothesis of the rarefaction and condensation of air or water vapor. The very first thinkers thus used the method of hypotheses, and the very first speculations were, logically considered, boldly conceived hypotheses.

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[1] John Dewey, *How We Think*, especially chap. vi. Cf. Huxley's essay on "The Method of Scientific Investigation" for a similar discussion.

§ 3. There was also another influence at work. Thales had brought the beginnings of scientific mathematics from Egypt and the East, and Greek genius developed a purely theoretical and deductive mathematics out of the rules of thumb and business practice that Eastern peoples had discovered. Because mathematics is of such great use in practical life and because its development demands only simple apparatus, the diagram or counting-board, it very early got a start over other sciences, and so acquired a dominance over men's minds far greater than that exercised by any other sphere of scientific interest. Mathematical demonstrations have an immediate claim to cogency upon everyone who understands them—in this respect they are very different from hypotheses, which require consideration and much thought before they can be properly accepted. Mathematics proceeds, by the process of deduction, from accepted propositions, axioms, and definitions, to other propositions which are as yet uncertain. Because of the superior development of mathematics and because of its immediate cogency, it is not surprising that philosophers should have modeled their reasonings upon that of mathematics. So we find in philosophy the rise of the deductive ideal. Philosophy is often reflection upon concepts developed by other sciences; hence it was natural that philosophy should have used the method of the earliest established science—mathematics.

It is characteristic of rationalism that the first rationalist, Parmenides, should have inaugurated the use of the deductive method (although we cannot be sure that Pythagoras before him had not previously used that method). When Parmenides put the fundamental principle of his philosophy into the form of an argument from the inconceivability of nonbeing to its nonexistence, he was using a simple deductive argument, which can easily be put into syllogistic form. His disciple, Zeno, invented the game of dialectical question and answer, in which

Socrates later showed himself such a master. This method of philosophic debate proceeds from a definition or premise, which is tentatively accepted by both parties in the debate, to its logically deduced consequences, and is thoroughly deductive in spirit.

Nevertheless, the older inductive method as used by the Milesians continued to be influential. Speculation justifies itself most naturally by the way in which it explains what is already known—a method of arguing that is plainly not deductive but inductive. The physical theories of the Greeks were mainly bold speculations that were essentially unverifiable, and the proof given, when any was adduced, was of the same type as that used by the Milesians.

§ 4. Socrates, as even Xenophon admits, was deeply versed in the mathematics and science of the time.[1] Plato pictures him in the *Meno* as demonstrating a geometrical proposition. Furthermore, he made a strict separation between what is demonstrated, which he called "knowledge," and what is not proved, and hence is only "opinion." He confessed that he himself had little or no knowledge. In the *Meno*, a geometrical demonstration is stated to be "knowledge"; thus we see what was Socrates' ideal of philosophic method—the deductive method modeled on mathematical demonstration. His way of dealing with his self-conceited contemporaries, getting them to venture a definition and then criticizing that definition, is also essentially deductive, for it is by drawing or deducing the consequences of the definition that it is shown to be justified or the reverse. When Aristotle says that "there are two things that one would rightly attribute to Socrates: inductive reasoning and universal definition,"[2] we have no right to interpret "inductive" in our sense of the word. Comparison with what

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[1] *Memorabilia* IV. vii. 3–5.

[2] *Metaphysics* xii. 4. 1078*b*.

Aristotle says elsewhere shows that by "inductive" he meant exactly the process of eliciting a knowledge of ideas such as is illustrated by Socrates' geometrical demonstration in the *Meno*.[1]

In the *Phaedo* is given a splendid example of Socrates' method of proof, which shows it to be deductive—Socrates and his interlocutors agree upon the existence of the ideas and the participation of things in them, and then he proceeds to deduce the immortality of the soul from the idea of life as being the essence of the soul. In that dialogue he also states his method of testing the truth of fundamental principles—by deducing the consequences of a principle and seeing whether they are self-contradictory or not; if they are found to be self-consistent, and the principle is still challenged, it must be deduced from a still higher and mutually accepted principle.[2]

In the *Republic* we are further enlightened as to the manner of obtaining truth. Truth is imparted to all objects by the idea of the good, which is both the cause of truth and of science.[3] In ordinary philosophizing, proof and conviction of truth can only be attained by deduction from principles which are accepted by both parties to a debate—that is one reason why Plato wrote *dialogues*, for only in a dialogue is it possible to use the method of deduction fruitfully without leaving ultimate premises hanging in the air; ultimate premises can, however, be accepted by both parties to the discussion, and conviction will then ensue by the use of valid deduction. Yet no matter how careful we are in warning our opponent not to accept a principle lightly, he may nevertheless be mistaken in doing so; therefore this method of dialectical debate is not ultimately justifiable. Hence there must be deduction from an ultimately true principle—which, for Socrates in the *Republic*, is the idea of the good.

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[1] See A. E. Taylor, *Varia Socratica*, pp. 72, 78; J. Burnet, *Greek Philosophy*, I, 158; H. H. Dubs, "The Socratic Problem," *Philosophic Review*, XXXVI, 287 ff.

[2] *Phaedo* 101E.

[3] *Republic* vi. 508E.

In the *Republic* we also find Socrates' ideal of a deduction of the sciences. The special sciences are founded on "hypotheses," or, as we should say, postulates. Socrates gives geometry as an example—he is conceiving of all science on the analogy of mathematics, as deduced from a set of postulates. Nevertheless a discipline which gives no account of its primitive propositions cannot be a science, for it cannot produce knowledge, but only opinion, since its first principles are not known as certain, and so the conclusions and intermediate steps are "constructed out of he knows not what." Then it is the business of philosophy or "dialectic" to demonstrate the primitive postulates of the sciences from the one ultimate principle.[1] This deductive ideal of science and of deduction from a single principle has had immeasurable influence upon later philosophies.

How, then, is this ultimate principle to be known? Socrates can only define it in negative terms. Clearly it cannot be demonstrated, for it is the source of all demonstration. It is ultimately known by a mystical procedure—when it is directly perceived, it carries its own evidence within itself. Yet this knowledge of the idea of the good must necessarily be expressed in intellectual terms; for,

> Until a person is able to abstract and define the idea of good, and unless he can run the gauntlet of all objections, and is ready to disprove them, not by appeals to opinion, but to true existence, never faltering at any step of the argument—unless he can do all this, he apprehends only a shadow . . . . and not . . . . knowledge.[2]

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[1] *Ibid.* vii. 533C, D.

[2] *Ibid.* vii. 534B.

Since Socrates was unable to define and abstract the idea of the good in this fashion, but could only describe it by analogy with the sun, it is plain on his own statement of the methodology of science that he had no knowledge; and he recognized that fact by confessing his ignorance to anyone who challenged him. These methodological considerations underlay Socrates' irony.

Socrates is thus pictured as accepting the deductive ideal whole-heartedly and making it the basis of all knowledge. He also saw the great difficulty of deduction—its inability to demonstrate its ultimate premises. But with an optimism characteristic of the man who spent his whole life in the endeavor to persuade his fellow-countrymen of their ignorance, he was content to state his ideal and his inability to measure up to it, in the hope that someone would find a way to solve his difficulty. Few of his disciples were as modest and as mercilessly logical as he.

We may be sure that the deductive ideal ascribed to Socrates also represents that of Plato. We know that he was also deeply interested in mathematics; the revision of the then accepted first principles of mathematics, which had been made necessary because of the criticism of Zeno, was largely done in the Academy.[1] Plato himself made important mathematical discoveries; mathematical concepts were fundamental in his metaphysics. The method of definition by division, which is humorously illustrated in the *Sophist*, is essentially deductive; it is of the same nature as the deduction of an ellipse from the "general conic." Deduction remained the way to knowledge.

But there was also another motif at work. The natural psychological method of solving problems, which had been used unconsciously by the first philosophers, could not be eliminated. Socrates' method of dealing with a disputed definition was not merely by deducing self-contradictory conclusions from it, which is a purely deductive procedure; he also disproved a definition by deducing from it conclusions that failed to correspond with recognized facts. Such an appeal to facts is not wholly deductive; it is also inductive. Socrates did not, however, recognize that herein he was using a different method, since deduction was involved in his procedure, and he probably thought that this was merely another use of the deductive method.

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[1] A. E. Taylor, *Plato*, p. 290.

Plato likewise did not confine himself to the deductive method. He recognized the necessity of "saving the phenomena," i.e., of proposing hypotheses that would account for the complexity of appearances—he is said to have proposed to the Academy the problem of "saving the phenomena" of the planetary motions. That problem meant the propounding of a simple hypothesis that would account, on the basis of order and regularity, for the apparently irregular motions of the planets. This task is clearly inductive, though deduction is an essential part of it; for the "appearances" must be deducible from the hypothesis. Whether Plato recognized this method as more than deduction we do not know.

We do know, however, that Timaeus the Pythagorean makes a strict division between the two worlds of being and becoming. Knowledge is only possible when dealing with the fixed and unchanging world of being; of becoming, only probability can be attained.[1] How far we may accept the words of Timaeus as representing Plato's own opinion is doubtful; yet this utterance may well represent Plato's attitude to the sciences of biology, etc., which were being developed in the Academy—not being deductive, they give only probability. This attitude is the same as the attitude of logicians to natural science down to this day, namely, that deduction alone leads to certain knowledge, so that natural science obtains only probability.

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[1] *Timaeus* 29.

§ 5. With Aristotle's discovery of the syllogism, formal logic begins. The syllogism was not, of course, utterly unlike anything previously known; it was a development and systematization of the methods of reasoning used by the Greek sophists and rhetoricians. Aristotle early achieved fame as a teacher of rhetoric, and set himself to the task of making argumentation a science. He then had to distinguish between correct and incorrect methods of reasoning; the syllogism was the result. His classification of fallacies was taken from the specious arguments used by the sophists of the time. Aristotle's discovery was not merely an invention of his own, but also a systematization of the practices of his predecessors. The *Topics* shows his familiarity and skill in the art of disputation, and that he knew how to secure rhetorical victory as well as scientific accuracy.

Aristotle was a systematizer—he gathered together into a system the discoveries made by his predecessors, together with his own discoveries. Though he was a competent mathematician, he was not an original discoverer in the field of mathematics, such as Plato. No one who had spent years in the school of Plato could have escaped the powerful influence of the mathematical, deductive ideal. Hence for Aristotle, as for Socrates and Plato, deduction is the way to knowledge and science; nothing else can give accurate knowledge. But Aristotle's chief interests were not in mathematics; they were rather in metaphysics and the natural sciences, especially in physics and biology. Mathematical physics had not yet been invented; the physics of the day, like biology, was entirely non-mathematical. Hence it is not surprising that Aristotle should have held that all logical (deductive) reasoning could be put into the form of the syllogism which he had invented. His attention was not upon mathematics, which was the only form of scientific inference at the time that could not have fitted easily into the Aristotelian syllogism; possibly he even assumed that most of that science, too, with a little pressing, could be fitted into the syllogistic form. Just as J. S. Mill thought that all empirical, inductive inference could be fitted into the five canons which he had stated, so Aristotle believed that all inference, even that of mathematics and induction, could be fitted into the Aristotelian syllogism.

Not only did Aristotle discover the form of deductive inference, the syllogism, but he also saw the problem which it presented—that of the ultimate premises. He saw clearly that deductive demonstration could not be infinite, for if the chain of prosyllogisms were infinite, demonstration could never be accomplished, and would therefore be impossible. Hence, in order to have demonstration and knowledge, there can be only a finite number of prosyllogisms; consequently there must be ultimate premises, which cannot be demonstrated just because they are ultimate. Then what validity have these ultimate premises? This was the great problem for Aristotle and has been the great problem for rationalism ever since. Aristotle's conception seems to have been that each of the sciences has its ultimate principles or axioms; hence it is the possession of a set of such ultimate principles or ἀρχαί which distinguishes one science from another.[1] Then how can these ἀρχαί be established securely?

The answer which Aristotle gave to this serious problem is so well known that it need only be outlined here. Historically, ultimate premises come from experience. The ultimate premises of a science are discovered by being analyzed out of the material of that science.[2] Yet it "is not possible to have scientific knowledge through sensation."[3] How, then, is any universal proposition to be proved? Aristotle invented what has since been called induction by complete enumeration or perfect enumeration. A proposition is shown to hold of each member of a group, and hence of the group as a whole. Later logicians have shown the impossibility of using this method literally in concrete situations, because no enumeration of concrete particulars can be completed unless its field is first artificially limited. It is doubtful how far this method was thought by Aristotle to assure knowledge, for it can only give scientific knowledge through sensation, the possibility of which he denied. Furthermore, this sort of proof would not be what Aristotle was thinking of when he spoke of demonstration as being "of such things as are essentially present with things."[1] Demonstration, according to Aristotle, is not attained by predicating anything whatsoever of something else, but by using as the middle term that which is already present *in essence* in the subject, and thus connecting it with the third term. In using induction by complete enumeration, we cannot be sure that we are dealing with the essence, for our middle term may be an accident; thus we should not properly have a middle term. Hence the inductive syllogism exists merely because we are ignorant of the major premise; "to nature the syllogism produced through the middle is prior or more known, but to us that by induction is more evident."[2]

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[1] *Posterior Analytics* I. xxviii. 1.

[2] *Ibid.* I. xxx. 2.

[3] *Ibid.* I. xxxi. 1.

If, then, even the inductive syllogism does not give knowledge, how can knowledge be obtained? In this situation, which threatens bankruptcy to philosophy, because philosophy has relied upon the deductive method, Aristotle did what Kant did in a similar emergency; he turned to psychology for help in his difficulty. He postulated a special faculty, that of intuitive reason, νοῦς, which is confined to gods and men, and which provides us with the infallible major premises that are first perceived in experience or by enumeration. His proof of the existence of this faculty is slight, but by its means he succeeded in saving himself from logical bankruptcy.[1]

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[1] *Posterior Analytics* I. xxii. 5.

[2] *Prior Analytics* II. xxiii. 3.

Had there been a set of ultimate principles which are given by this faculty of intuitive reason, Aristotle ought to have given a list of them, but he nowhere does so. He merely states the one principle which he considers the most certain of all—the law of non-contradiction.[2] He does not, however, show that anything else can be deduced from this law. Fichte attempted to deduce all philosophy from the law of identity, but Aristotle leaves the law of non-contradiction hanging. He does, however, show that the laws of non-contradiction and of excluded middle are involved in the making of any significant assertion whatever.[3] This is not a positive argument for their validity, as Aristotle recognizes; but it does show that they are among the most fundamental of propositions; that proof was a sufficiently persuasive argument to keep any but the keenest of his successors from questioning Aristotle's logic.

Thus Aristotle first formulated the deductive method in the form of the syllogism, and at the same time propounded the great problem for logic—if demonstration is by deduction, how can the necessary ultimate premises be validated? It is this unsolved problem that has been responsible for most of the difficulties and aberrations of philosophy since his time. For philosophy was left without a sure method of attack upon its problems and without any "knowledge," in the sense given that word by Socrates and Plato, from which to demonstrate its conclusions.

§ 6. The skepticism which underlay the succeeding development of philosophy in the ancient world was undoubtedly motivated by this difficulty of Aristotle's. This skepticism centered about the impossibility of getting veridical sense-perceptions and about the difficulty of getting ultimate premises. If we reject Aristotle's dogmatic assertion of a special infallible faculty that gives ultimate truths, then we are thrown back upon experience for these necessary ultimate premises, as Aristotle recognized. Thus the question arises, How is it possible to obtain absolutely certain sense-perception? If such veridical sense-perception is not obtainable, knowledge is impossible and skepticism is inevitable. The impossibility of obtaining certain and ultimate premises is the fundamental argument of the skeptic. If Aristotle had been able to establish irrefutably a set of ultimate principles, and to demonstrate that his metaphysical system could be deduced from those principles with inevitable necessity, skepticism could not have raised its head.

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[1] Cf. *Posterior Analytics* II. xix; also F. C. S. Schiller, *Formal Logic*, p. 235.

[2] *Metaphysics* IV. 1005*b*. 18.

[3] *Ibid*. IV. ii.

§ 7. Skepticism is, however, only negative; men need a positive guide for life, which they sought in philosophy. There were only two alternatives left to a philosophy which should proclaim any positive teaching, since rational induction was unknown and deduction could not establish its fundamental principles; these were dogmatism and mysticism. Both of these alternatives were embraced by the positive philosophies that dominated the later period of the ancient world. While Stoicism utilized the Aristotelian logic and made it the propaedeutic to the study of Stoic philosophy, yet the Stoic could only assert dogmatically the underlying principles of his philosophy. For fundamental beliefs there was no proof. Epicureanism was likewise a dogmatism. A dogmatism cannot ultimately withstand the force of philosophic criticism; so we find, first, syncretism, a dogmatic attempt to combine the good points of irreconcilable views in the endeavor to meet the criticisms of the rival schools, and, finally, both Stoicism and Epicureanism succumbing to the overwhelming skepticism of the time.

Plotinus found certainty in the mystic revelation. Considered from the viewpoint of logical analysis, the mystic revelation is on the same plane as the infallible intuitive reason of Aristotle. The mystic has, however, this further advantage: his trances are psychologically distinguished by various phenomena that tend to inculcate in the spectator respect for the mystic revelation and that also produce in the mystic a feeling of immediate certainty. Therefore, it is not surprising that neo-Platonism should have had a great vogue and a tremendous influence, even upon Christian philosophical theologians. But, as we have previously shown, the mystic revelation only gives the mystic certainty to the person who has that revelation; whoever else accepts the deliverances of the mystic can only do so on the authority of the mystic; so the logical basis of knowledge shifts, for him, from mysticism to authoritarianism. On the other hand, if the believer should use any criterion whereby to judge the verity of the mystic's deliverances, such as the application of tests of sainthood, the basis of knowledge shifts again to some form of rationalistic deduction; but the ultimate question remains unanswered: Just how is it known that these tests are the *true* tests of sainthood? Thus Aristotle's problem of validating the ultimate major premise again confronts the investigator, and the only feasible answer is that he dogmatically accepts this test of sainthood! It is therefore not surprising that neo-Platonic mysticism should have developed a dogmatism in stating its philosophical system, for it could only be propagated by the uncritical acceptance of its teachings.

If mysticism attempts to avoid such authoritarianism, there is a further difficulty which confronts it. If the mystic trance is its own validation, then the believer in the authority of the mystic must accept the revelations of all mystics of whatever sort. But mystics have never agreed on any single deliverance: they have been theists, pantheists, and atheists; they have taught the positive as well as the negative theology; their morality has been thisworldly as well as otherworldly. In the ancient world this diversity of the mystic deliverances led to a special situation. All the religions of the ancient peoples were supposed to be founded on some sort of divine revelation —hence the neo-Platonist believer in mysticism had logically to accept them all. Consequently Jamblichus and others attempted to systematize and harmonize the mystically revealed religions of the world, and so developed a neo-Platonic superpolytheism. Once we accept uncritically the deliverances of the religious consciousness, the door is open to all sorts of superstition. Thus the most important characteristics of the ancient philosophies were the result of the logic of the theory of knowledge.

§ 8. Christianity was not at first a philosophy, but a way of life. To justify and defend itself, it had to dress itself in terms of the philosophy of the time. Consequently, the Christians took over the difficulties of the ancient philosophies in stating their beliefs in intellectual terms. Since the ancient philosophies ended in dogmatism, skepticism, or mysticism, it was natural that Christian theology should have taken the two of these which it could use, dogmatism and mysticism. The Catholic church looked askance upon any divergence from its faith, and hence subjected its mystics to tests before accepting their deliverances. We have seen that any rational test of mysticism becomes ultimately authoritarian, so it is not surprising that the Catholic theology should have based itself upon authority. There was no other basis upon which it could rest provided by the ancient philosophies in whose terms Christianity was expressing itself. Thus dogmatism was inevitable. Catholic theologians took over the Aristotelian logic as the best method of expounding and rationalizing their teaching, but when it came to the problem of establishing their ultimate premises, Aristotelian logic failed, and the only alternative was to accept them on authority. With its ultimate premises secured by the authority of divine revelation, and the remainder of its theology deductively demonstrated from these premises, the Catholic theology seemed to be in a philosophically impregnable position. The length of time during which this systematic authoritarianism has dominated the minds of men and the greatness of some of the minds who accepted this philosophy is a measure of the persuasiveness of such a foundation to intelligent thought.

There was, however, a difficulty. If the deliverances of the Bible, which was the greatest source of revealed truth, had been self-consistent and adequate to supply the major premises to answer all questions, Catholic authoritarianism might still be dominating all intelligent minds. However, even among the Catholic theologians there were differences—the nature of Christ, predestination and free will, the priority of reason or will in the divine mind, the relation of the church and the state—these and other philosophic problems could not be settled by quoting proof-texts. The writers of the Bible had not faced these problems, and the church Fathers disagreed. To remedy this defect in authority, the church developed the doctrine of the infallibility of the General Council, and later the infallibility of the Pope when speaking ex cathedra—but such authorities had necessarily to be cautious about expressing themselves on such complicated, difficult, and far-reaching questions. A rash decision might alienate the sympathy of intelligent men by being proved demonstrably false. This was the Damocles' sword inevitably hanging over a supposedly infallible authority. Hence such an authority could not promptly and efficiently settle disputed points.

Dogmatic authority can always be dogmatically questioned; and when the papacy was found sanctioning one wrong practice, the sale of indulgences, Luther found that his attack upon indulgences logically drove him to question the authority of the papacy. He, too, used the scholastic Aristotelian logic, and while, for himself, he substituted the mystical authority of his conscience for that of the pope, nevertheless his followers had no alternative, in the logic of the situation, except to fall back upon another infallible authority in order to secure a sure foundation for their reasoning. This foundation they found in the Bible. The Bible is not unambiguous, and so the dissensions among the Protestants, together with their dissatisfaction with the scholastic theologies, would have revived the ancient spirit of philosophical criticism, even if other influences, especially the rediscovery of the ancient philosophies, had not assisted in that development.

§ 9. The opening of the modern world is marked, not by a change in method, but by the acquisition of new knowledge. A new mode of architecture, gunpowder and a new method of fighting, the compass and the discovery of a new world, printing, paper, and the dissemination of knowledge, the telescope, the microscope, with their new revelations in astronomical and biological worlds—these and other inventions ushered in a new age and a new spirit of confidence in the power of man to deal with his world. It is the continual stream of world-transforming discoveries, such as the steam-engine, the telegraph, the telephone, the X-ray, electric light, the automobile, the aeroplane, that have made impossible for modern man any widespread skepticism of the possibility of knowledge. These inventions are to him the proof that knowledge has been achieved and that the world is amenable to the action of the human mind. Whereas the ancient and medieval world expected no new discoveries and no progress, and was content to believe that finality had been reached, the modern world is looking forward to continued advance in knowledge.

This sudden development was not brought about by a new method of discovery. The most important of the practical inventions in the opening years of the new period were not discovered by Europeans, but were imported from other centers of culture as the result of improved means of communication. Alchemy, the parent of chemistry, the compass, gunpowder, printing, and paper came from China; the Arabic (Hindu) number system, a developed mathematics, and the literary remains of the ancient world came from Mohammedan countries. These discoveries were largely responsible for the initiation of new movement in European thought, but they were not the products of the new European culture.

The science which had made the greatest strides in the ancient world, and which developed most between the time of Aristotle and Copernicus, was the deductive science of mathematics. The use of Arabic numerals and algebraic symbols gave to mathematics an ease of calculation unknown to the Greeks. Consequently it was natural that modern science should have attached itself to mathematics and should have been profoundly influenced thereby.

§ 10. It is a striking fact that the modern thinkers who inaugurated modern science had no consciousness that they were using any different method from that of the ancients. Copernicus was primarily a mathematician who was dissatisfied with the Ptolemaic astronomy because of its cumbrousness, and sought to reform it so as to obtain greater mathematical simplicity and beauty. He found a method of mathematical resolution whereby he was able to eliminate a large number of the complex epicycles in the Ptolemaic system by changing the point of reference of the system.[1] Kepler was an enthusiastic Pythagorean and a creative mathematician who believed that Nature acted in a simple and harmonious manner in accordance with mathematical principles, and whose fertile imagination suggested to him various mathematical plans of the universe. Had it not been for the careful observations of Tycho Brahe, Kepler's astronomical discoveries might have ended with the *Mysterium Cosmographicum*, in which the Copernican theory was supplemented by the statement that "the distances between the orbits of the six planets then known bore a certain rough resemblance to the distances which would be obtained if the hypothetical spheres of the planets were inscribed in and circumscribed about the five regular solids properly distributed between them."[1] But the accurate observations of Tycho showed the impossibility of such a scheme; so Kepler, with inexhaustible patience and enthusiasm, set about to discover those mathematical relations that he was a priori certain were to be found in the operations of nature, and thus finally discovered his three famous laws. Kepler himself thought that truth is to be found in that supposition which is both more inclusive and shows a mathematical order and harmony where before there had been unexplained diversity, and is at the same time discovered in the facts themselves.[2] In reality, however, he was using a different method of proof, the same as that used by the earliest Greek philosophers in their physical speculations, and by common men everywhere in solving practical problems—that of assuming a hypothesis and seeing how the consequences of that hypothesis fit in with the observed facts. Where Kepler differed from these early speculators was not in method, but in possessing a far more powerful means of drawing the consequences of his hypothesis—he had a mathematics more developed than any in ancient Greece and an array of facts and orderly observations, such as no astronomer before him had possessed. His association with the great Danish observer had taught him the importance of *accuracy;* so we find Kepler, in his laborious search for the truth, discarding hypothesis after hypothesis with amazing candor. Yet the moving force in his protracted labors was not a belief in the adequacy of inductive method, but his conviction that the universe is fundamentally mathematical.

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[1] E. A. Burtt, *Metaphysical Foundations of Physics*, chap. ii.

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[1] *Ibid.*, p. 51.

[2] *Ibid.*, pp. 54 f.

Galileo was also fundamentally a mathematician, although he also had a remarkable talent for practical mechanical inventions. His conception of his discoveries was that he was extending "the exact mathematical method into the realm of somewhat more difficult mechanical relations."[1] Galileo experimented only sufficiently to discover new relations; then went ahead to deduce mathematically their consequences, without feeling the necessity of experimental verification.[2] For example, he deduced from his law of falling bodies the conclusion that the path of a projectile is a parabola—although in most cases it is far closer to a semicircle. But the composition of the accelerated motion produced by gravity with the uniform motion produced by gunpowder requires the path of the projectile to be a parabola. Galileo therefore took the parabola as a demonstrably correct statement of its path, without experimenting as to its true shape. He forgot the resistance of the air. For him, experimentation was merely a means of discovering the relations involved, and of confuting his opponents. Galileo was probably giving his own view of his method when he wrote of Aristotle:

> I do believe for certain, that he first procured, by the help of the senses, such experiments and observations as he could, to assure him as much as was possible of the conclusion, and that he afterwards sought out the means how to demonstrate it; for this is the usual course in demonstrative sciences. And the reason thereof is, because when the conclusion is true, by the help of the resolutive method, one may hit upon some proposition before demonstrated, or come to some principle known *per se;* but if the conclusion be false, a man may proceed *in infinitum*, and never meet with any truth already known.[3]

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[1] *Ibid.*, p. 62.

[2] *Ibid.*, p. 65.

[3] *Two Great Systems*, p. 37; quoted in Burtt, *op. cit.*, p. 67.

Thus the proof of his discovery was by the method of mathematical analysis, and he thought of physics as a deductive science.

The reason that Galileo did not emphasize experiment as the guide to knowledge was, very likely, to be found in his recognition of the fact that many of his laws were necessarily and inevitably unverifiable. Thus, in dealing with the first law of motion, that a body continues in uniform motion unless compelled to change its direction or velocity by other forces, Galileo found that no ball would roll on indefinitely, but that the smoother the ball and the smoother its path, the longer it would roll; hence he assumed that if the ball and path were perfectly smooth, i.e., frictionless, its motion would never cease unless disturbed. Similarly with the inclined plane upon which he illustrated his law of falling bodies—the smoother the ball and path, the more closely its motion conformed to his mathematical law—no exact verification of the law was possible. Therefore, Galileo naturally felt that mathematical deduction was more accurate than experimentation. Yet he was alive to the necessity of getting from experience a true conception from which to start deduction; man must ferret out the secrets of nature, only then should he proceed to deduce their consequences. Thus Galileo thought of his method as primarily deductive. Often he only performed experiments to meet the attacks of opponents; hence it was fortunate that he had strong opponents, for they forced him to depend on experiment as well as upon deduction. He had no ideal of careful examination of his ultimate premises such as Aristotle had urged, for he was not awake, as was Aristotle, to the utter dependence of deduction upon its ultimate premises.

§ 11. When the founders of modern science saw their own method as deductive demonstration, it is not surprising that such an opinion should have persisted in the minds of their successors, the natural scientists, who are men primarily engaged in the business of discovery, rather than in reflection upon what they are doing. Descartes' theory of vortices and Hobbes's theory of the nature of the air, both of which are now relegated to the scientific lumber-room, were the natural result of such an overemphasis upon deduction in science. It was Boyle, himself not a profound mathematician, who did the most to bring science back to its ideal of faithfulness to fact. He overthrew Hobbes's physical philosophy by experiment, and, after him, no important thinker dared again to propose a scientific theory without appealing to exact experimental verification. But, even for Boyle, the criterion of truth is primarily rational. "Experience is but an assistant to reason."[1] The inevitable presence of deduction as an essential part of induction misled scientists and philosophers into thinking that the discovery of knowledge is primarily deductive.

Isaac Newton has probably had more influence in determining the method of scientific investigation than anyone else; his influence upon methodology has not been due to any conscious statement of method, but rather to an imitation, by other scientists, of the procedure that, in Newton's case, had so successfully and patently unlocked the secrets of the physical world. The conviction that Newton's "method of investigation was logically perfect"[2] was engendered, by its success, in the minds of many persons who were incapable of correctly analyzing that method, but who could, nevertheless, correctly apply it to problems which were different in nature. What that method really was will be stated in the next chapter; here we shall only note that Newton entirely misunderstood the nature of his own procedure. He correctly realized that he had achieved knowledge, and attempted to distinguish between what had been proved and what was still doubtful—his famous attack upon hypotheses represents his conviction of the necessity of making that distinction. His saying, "I frame no hypotheses," which he could not even consciously carry out,[1] represents his conviction that he asserted nothing to be true without adequate proof, and that scientists should not confuse the proved law with the still unproved hypothesis. His indignation at the charge that he made hypotheses was an indignation at the supposition that he had failed to *prove* his experimental laws. But Newton did not realize the ambiguity of the word hypothesis as standing either for *any* supposition or ultimate premise, or for an *unproved* supposition, and so he became involved in many disputes as to the nature and validity of his doctrines. Consequently he finally decided to banish all hypotheses from his works; in the *Opticks*, where he could not avoid lengthy speculations, he put them in a separate part of the book.

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[1] *The Works of R. Boyle*, V, 539; quoted in Burtt, *op. cit.*, p. 164.

[2] Jevons, *Principles of Science*, p. 607.

Newton's rules of reasoning are notoriously inadequate as a statement of his method of procedure. In view of his great mathematical attainments, it is not surprising that he should have conceived of his procedure as wholly deductive in its nature.

> The proper method for inquiring after the properties of things, is to deduce them from experiments.[2]
>
> To derive two or three principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy.[3]

Newton thought of himself as deducing his first principles from experiment (which is impossible, since deduction is only

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[1] Taking "hypothesis" in the sense of a supposition, not as a mere speculation.

[2] *Opera*, IV, 320; quoted by Burtt, *op. cit.*, p. 212.

[3] *Opticks*, p. 377; quoted by Burtt, *op. cit.*, p. 219.

Descartes started philosophy along the same deductive method that brought about the failure of the ancient philosophy. The continued acquisition of knowledge by natural philosophy prevented the prevalence or permanent influence of skepticism in the modern world; but the history of the English school of philosophy from the time of Descartes to Kant is a record of the disproof of one after another of Descartes' primary assumptions, until it ended in the skepticism of Hume.

§ 13. The Continental school, on the other hand, sought to correct and emend the errors in Descartes' method so as to produce a permanently true system of philosophy. The devotion of Spinoza to the mathematical deductive ideal is shown throughout his writings. Wherever he needs an illustration regarding the nature of truth, he takes one from mathematics, especially from geometry. He put his greatest work, the *Ethics*, into the form of a geometrical demonstration. Of his four modes of perception (or knowledge), the first is the method of authority; the second, that of empirical knowledge, whence comes "nearly all things that are useful in life";[1] the third is deduction, but without an adequate understanding of the grounds of that deduction, as when tradesmen find the fourth proportional by rule, having forgotten the deduction of the rule; and the fourth, or most perfect and sole adequate method of knowledge, is deduction with a knowledge (and hence certainty) of its ultimate premises, as when mathematicians know that numbers are proportionals "by conviction of the proof of Proposition 19, Book 7, *Elements of Euclid*."[2] Thus Spinoza committed himself to the deductive method; he went so far as to say to the skeptic that if he is not convinced of the existence of first truths and of the truth of their deduced consequences, he has no knowledge at all and becomes a mindless machine![3]

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[1] *De Intellectus Emendatione*, iv, sec. 20.

[2] *Ibid.*, sec. 23.

[3] *Ibid.*, vii, sec. 48.

Having accepted the deductive method, Spinoza's problem is to secure his "first truths." He argues that "falsity consists in this alone, that something is affirmed of some other thing which is not contained in the conception of that of which we have formed—as motion or rest from a semicircle,"[1] i.e., that a false idea is one which cannot properly be deduced from the idea of that which it is about. Thus falsity is due to an invalid deduction; consequently, simple ideas cannot be false, since they are not deduced at all, "whence it follows that simple thoughts cannot but be *true*" and "wherefore we may at our own free will form simple ideas without any danger of error."[2] Spinoza later limits this definition of ultimate truth to simple ideas formed by the understanding, though he does not say how to distinguish ideas formed by the understanding from those formed by other faculties. The truth of ideas is guaranteed by the fact "that when the mind pays attention to any thought in order to examine it, and deduces in good order from it whatever is to be deduced, if it is false, it detects the falsity; but if it is true, then it proceeds happily without any interruption to deduce true things from it"[3]—it thus belongs to a self-consistent system. Then non-contradiction is a guaranty of truth, and an "eternal truth" is one which is guaranteed by the law of non-contradiction; that is, an impossible thing by its nature implies a contradition if it exists, and a necessary thing by its nature implies a contradiction if it does not exist.[4] Having thus assumed the deductive method, the law of non-contradiction, the existence of true ideas, and simplicity as the guaranty of ultimate truth, Spinoza goes on to state and assume his simple ideas, and to deduce his system therefrom.

§ 14. It was to be expected that Leibnitz, the creative mathematician, the founder of symbolic logic, the many-sided genius, should similarly have made deduction the fundamental means of knowledge. Such was indeed the case; furthermore, he proposed to demonstrate many things which had previously not been thought susceptible of deductive proof. In fact, he believed that all paralogisms are merely erroneous deductions, and looked forward to the time when it should be true that

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[1] *Ibid.*, ix, sec. 72.

[2] *Ibid.*

[3] *Ibid.*, xiv, sec. 104.

[4] *Ibid.*, viii, sec. 53.

> when controversies arise, there will be no more necessity for disputation between two philosophers than between two accountants. Nothing will be needed but that they should take pen in hand, sit down with their counting-tables, and (having summoned a friend, if they like) say to one another: *Let us calculate.*[1]

This calculation was, of course, deductive reasoning, whether of the mathematical or the Aristotelian sort.

Then how are the ultimate premises of this calculation to be validated? Leibnitz nowhere attempted to set down his system, and so nowhere faced this question with persistent attention to its full implications. He never pursued it even as far as did Spinoza in *De Intellectus Emendatione*. Consequently we find only scattered and shallow statements concerning this most important problem. We must remember that mathematics, especially Euclidean geometry, was still considered to produce absolutely certain and eternal truth, and it was recognized that mathematics began by asserting axioms and definitions. Furthermore, Leibnitz recognized two kinds of truth, eternal and necessary truth, and truth of fact, which latter is contingent and merely probable. He recognized that men are nine-tenths empirics, and that if opinion, based on probability, is not admitted as knowledge, "nearly all historic knowledge and many other kinds will fall."[2] But certainty is only found in eternal and necessary truth, or what he called truths of reason.

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[1] Quoted by Latta, *Leibnitz, The Monadology, etc.*, pp. 85 f.

[2] *New Essays*, Bk. IV, chap. ii, sec. 14.

Eternal and necessary truths are grounded upon the principle of contradiction. Falsehood is that which involves a contradiction, and truth is "that which is opposed or contradictory to the false."[1] He gives as examples of primitive necessary truths the principles of identity (in various forms), of contradiction, and of excluded middle.[2] We must, however, remember that no single and simple (not complex) proposition can produce a contradiction, not even such a one as "Squares are round." Contradiction can only occur between two propositions, such as between the proposition we have given and the definition of a square. This was recognized by Spinoza when he stated that truth belongs to a self-consistent *system*. Leibnitz had his own way of getting out of this difficulty. For him, all predicates are contained in the intension of the subject and may be discovered by analysis. Thus the term "square" would mean, for Leibnitz, the definition of a square, and the proposition would become, "Equilateral quadrilaterals are round." Even in this form there is no contradiction expressly to be found, and further manipulation is necessary to bring out the contradiction; not until we take the word "square" to be literally the system of geometrical meanings implied by the term "square," including the definition of a straight line as distinguished from the definition of curved lines, and including the definition of contradiction, does that single proposition become self-contradictory. Thus Leibnitz's definition of truth implies his logical doctrine that all predicates are contained in the subject, including all the predicates of those predicates, so that every term is itself a whole system of ideas and propositions. Furthermore, he had not realized the problem presented by mutually incompatible but self-consistent (geometrical) systems.

[1] *Monadology*, sec. 31.

[2] *New Essays*, Bk. IV, chap, ii, sec. 1.

For Leibnitz, eternal and necessary truths are innate, not in the sense of Descartes or Locke, but, since the monad mirrors the universe, all ideas of the monad are innate. In the beginning they are not clear and distinct; the recognition of their self-evidence is the result of a process of development from confusion to clearness. "The decisive proof of these principles consists in showing that their certainty comes only from what is in us."[1] That Leibnitz is thinking, as did the other rationalists, of the sort of certainty given by the simple and clear first principles of mathematics is shown by the illustration which he uses—the demonstration by Socrates in the *Meno* of a geometrical proposition. "The principle of contradiction is by itself sufficient for the demonstration of the whole of arithmetic and geometry, that is to say, of all mathematical principles,"[2] and so the principle of contradiction suffices to demonstrate all eternal truths of reason. Leibnitz did not face the problem of ultimate premises in its full severity; he assumed the law of contradiction to begin with, and did not seek to validate it; to him it was self-evident and innate.

The philosophers of the seventeenth century were still too close to the scholastic period, in which deductive reasoning could be used without difficulty, because ultimate premises were guaranteed by authority. Once the acceptance of authority was removed, the weaknesses of this method of reasoning were sure to appear. Still more important, mathematics, especially geometry, had been too long established as a secure and certain science for them to realize its ultimate weakness; they were too far removed from the reconstruction of the foundations of geometry that occurred in the early years of the Academy. Thus they were unable to subject mathematics to an adequate methodological criticism; without criticism they took it as an example of certain and eternal truth and so modeled their philosophical method upon it. But once the critical spirit was fully aroused, nothing could prevent the fall of philosophies established upon such unstable foundations.

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[1] *Principles of Nature and of Grace*, sec. 5.

[2] "Second Letter to Clarke," quoted by Latta, *Leibnitz, Monadology, etc.*, p. 237, note 54.

§ 15. The rationalism of the English empirical school appears in its acceptance of the deductive method; its empiricism appears in its rejection of innate ideas. The attractiveness of the deductive method is well illustrated by the experience of Hobbes. He had completed a university career without becoming acquainted with mathematics; it was not until he was forty years old that, on the Continent, he became acquainted with mathematical demonstration. Its compelling power and immediate certainty produced such great enthusiasm in his mind that he thereupon resolved to use the demonstrative method alone in his reasonings. The reaction against the scholastic syllogism resulted, in England as on the Continent, not in a turning away from deduction, but in the substitution of mathematical or other forms of demonstration for the demonstration of the Aristotelian syllogism.

§ 16. It has often been a source of wonderment that Locke should have made the first book of his *Essay* an attack upon innate ideas. The explanation is simple—he is rejecting any uncriticized ultimate premises of deduction. He is refusing to smuggle in, by an illegitimate and uncritical process, any premises which he cannot justify. Locke misconceived the nature of the rationalists' innate ideas, but when we consider the function which innate ideas played in the rationalistic philosophies, that of providing first principles from which the rest of the philosophy could be deduced, Locke's intent becomes plain. He would not take any premises without criticism. This motive is also behind his virulent attack upon mystical revelations in his chapter on "Enthusiasm,"[1] and is likewise shown in his discussion of axioms.[1] He was not ready to eliminate all axioms, for that would leave him without any possibility of demonstrating anything, and mathematics was plainly a demonstrative science, so he allows a few self-evident truths: all propositions of identity or diversity, of the form x is x or x is not y, are equally self-evident, as immediately perceived by the mind without the help of any sensation; of relation, he admits mathematical axioms; but of coexistence (physical relations) he admits only one, that of the impenetrability of matter, and possibly a very few others; and of real existence there are no self-evident propositions. Thus Locke eliminated all the deductive physics and metaphysics of the Continent by the simple expedient of rejecting the foundations on which they were built.

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[1] *Essay on the Human Understanding*, Bk. IV, chap. xix.

Locke was not a mathematician, but a practical man, and practical men have never depended on demonstration alone. While he held that the highest (psychological) degree of knowledge consists in intuition (or self-evident truths, as limited in the foregoing), and the next degree is demonstration,[2] and while he asserted that anything that comes short of these "is but faith or opinion,"[3] and is only probable,[4] yet he also called it "knowledge," and asserted that it proves the existence of things external to us.[5] This is, of course, an inconsistency, the result of raising a larger number of problems than he could think through. Indeed, Locke's primary purpose in the *Essay* is to investigate the nature of human knowledge[6] and to attempt to discover its validity by a consideration of its source and its character—his primary purpose is psychological rather than logical—as he himself said, "If I have done anything 'new,' it has been to describe to others, more particularly than had been done before, what it is their minds do when they perform that action which they call "knowing.'"[1] Such a psychological investigation cannot be carried on by a deductive process, but by an analysis of experience; consequently a large part of Locke's reasoning is not deductive.[2] He was not the last to make logical principles for the attainment of truth depend upon psychological truths previously established. Only an overfull or confused mind could have made such an error. Kant followed him in this attempt to found logic upon psychology; the structure of the *Critique* is enough to illustrate Kant's confusion; since that time, Kant has had a host of imitators.

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[1] *Ibid.*, Bk. IV, chap. vii.

[2] *Ibid.*, Bk. IV, chap. xvii, secs. 14 and 15.

[3] *Ibid.*, Bk. IV, chap. ii, sec. 14.

[4] *Ibid.*, Bk. IV, chap. xvii, sec. 16; Bk. IV, chap. xv.

[5] *Ibid.*, Bk. IV, chap. xi, sec. 3.

[6] *Ibid.*, Bk. I, chap. i, sec. 2.

In spite of this non-deductive element in Locke's thought, whenever he comes to any matter which he considers important, he uses deduction as his method of proof. He seeks for "necessary connection or repugnancy"[3] between ideas; he proposes to put morality upon the same deductive basis as mathematics; and he demonstrates the existence of God. Mathematical deduction is for Locke the ideal method of advancing knowledge. He sees it need not be confined to quantity, and describes it as the comparison of "clear complete ideas under steady names."[4] Induction from experience "may procure us convenience, *not science*."[5]

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[1] Second Letter to the Bishop to Worcester, pp. 83 ff.; note to *Essay*, Bk. IV, chap. i.

[2] A surprising number of Locke's psychological reasonings are, however, deductive. For example, the statement, "All ideas come from sensation or reflection" (*Essay*, Bk. II, chap. i, sec. 2), is established from the major premise that the mind is a *tabula rasa;* it has ideas, hence these ideas must have come from without, i.e., from experience! It was the fallacious major premise of this piece of reasoning that Leibnitz attacked.

[3] *Essay*, Bk. IV, chap. iii, *passim*.

[4] *Ibid.*, Bk. IV, chap. xii, secs. 3, 6, 14.

[5] *Ibid.*, Bk. IV, chap. xii, sec. 10. Italics mine.

§ 17. There could be but one end to such a rejection of ultimate premises combined with an acceptance of deduction as the only method of science. Locke's partial rejection of self-evident truths was found to end, as a result of adequate criticism, in the rejection of all such self-evident truths; utter skepticism as to the possibility of knowledge would be the inevitable result if anyone was found to carry the empirical movement to its logical conclusion. Before this skeptical result was drawn, however, empiricism, in the person of Berkeley, developed a most interesting philosophy, in which the skeptical result was only half reached.

Berkeley's argument against the existence of material substance is easily seen to be a fundamentally deductive one. His major premise is the ambiguous statement, "The existence of a thing consists in its being perceived,"[1] which is taken as meaning that the existence of a thing can only be proved by its being perceived—a statement which is nowhere proved, merely taken for granted. The body of his argument is the proof of the minor premise that "material substance is not perceived." In the *Principles*, he directs his argument chiefly against the Lockian position that simple ideas are copies of reality, and against the supposition that primary qualities constitute the perception of matter; in the *Dialogues*, he also argues against the naïve view that all sensations constitute perceptions of matter. Of course, if the major premise is accepted, everything not immediately perceived becomes nonexistent. It is Berkeley's major premise that contains the crux of his argument, and it is surprising that the opponents of Berkeley should have directed their attacks against the minor premise or against other parts of the argument, rather than against the major premise. Perhaps they have done that because no opponent of Berkeley has been able to suggest any other criterion of reality to take the place of the one Berkeley assumed. After the severe criticism to which

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[1] *Principles*, Part I, sec. 2.

Hume subjected Berkeley's position, other idealists have been chary of taking precisely that position, yet they have accepted Berkeley's argument as establishing the nonexistence of the material world, and as paving the way for idealism. Berkeley's demonstration of the nonexistence of matter is the *negative* foundation of modern idealism, and his uncritical acceptance of the axiom that "the existence of a thing consists in its being perceived" is the nerve of that demonstration. Consequently it is highly important for realism that the neglected task of examining this axiom be undertaken. Yet how can we examine it unless we have something to put into its place? Without thus meeting Berkeley squarely on his own ground, realism cannot be established securely, for the Berkeleian argument will be left undefeated, in the rear, to capture any positions that realism may attempt to secure. These considerations point to an important task for current philosophy.

Berkeley's proof for the existence of God is likewise a deductive one. Its major premise is the unexpressed axiom of causality, that everything has a cause; consequently ideas must have a cause, which can only be other ideas, matter, my own spirit, or another spirit. The minor premise is that the cause can be neither matter (proved by his main argument for the nonexistence of matter), other ideas (proved by the inactivity of ideas[1]), nor by my spirit (proved by the independence of the ideas of sense from my will[2]); consequently ideas must be caused by an Other Spirit, which is orderly,[3] hence good, hence God. The very simplicity and brevity of Berkeley's arguments, as well as their deductive character, aid in making them extremely persuasive. But their purely deductive character robs them of all cogency once the major premise is challenged.

[1] *Ibid.*, sec. 25.

[2] *Ibid.*, sec. 29.

[3] *Ibid.*, secs. 30–31.

§ 18. Hume rejects the Aristotelian syllogism more emphatically than his predecessors; he denies that every proposition contains two ideas and asserts that it is possible to reason from one proposition to another without the mediation of a third, i.e., from particular to particular,[1] though he has no adequate justification for that view. He invents a new inductive logic and asserts that this logic is all that he thinks proper to employ in his reasoning.[2] But he recognizes that this inductive logic could not produce certainty, only probability; and, since Hume wishes to refute the older philosophies conclusively, he does not use this inductive logic explicitly in his reasoning. He is trying to convince his readers, and so has to use the same sort of reasoning that writers of the time used in argument—deductive reasoning. Since he believes that it is possible to infer one statement directly from another, he does not stop to state the major premises of his inference, not even as little as Berkeley, but leaves them unstated. An analysis, for example, of the argument concerning abstract ideas[3] shows that it is thoroughly deductive in character. He accepts the rationalistic and deductive criterion that impossibility proves nonexistence, and that conceivability establishes the possibility of existence.[4] He then argues that it is conceivable that instances of which we have had no experience will not resemble those we have had experience of, hence no actual experience can disprove a mere possibility of existence, and so possibility can only be limited to conceivability.[5] All reasoning is merely a comparison and a discovery of those relations which objects (terms) bear to each other;[6] if these relations are constant and unchangeable,

[1] *Treatise*, Bk. I, Part III, sec. vii, note; Selby-Bigge edition, p. 96, note.

[2] *Ibid.*, Bk. I, Part III, sec. xv.

[3] *Ibid.*, Bk. I, Part I, sec. vii.

[4] *Ibid.*, Bk. I, Part II, sec. ii; Bk. I, Part IV, sec. v, p. 250.

[5] *Ibid.*, Bk. I, Part III, sec. vi, p. 89.

[6] *Ibid.*, Bk. I, Part III, sec. ii, p. 73.

we have knowledge; but since it is conceivable that the relations should change, it is possible that they will change; hence no mere experience of unchanged relations can establish their unchangeability: then if relations are possibly inconstant, no statement can be more than merely probable. Hume's view that reasoning consists in a comparison of eternal relations is itself derived from the logic of the deductive method, although not necessarily from the Aristotelian syllogism alone.

Hume thus uses the deductive method to establish his conclusions as much as any other philosopher of the century. He was neither a mathematician nor a scholastic logician, so did not realize the true nature of deduction as depending upon ultimate premises, a fact which is evident in his treatment of the scholastic statement that all reasoning is *ex praecognitis et praeconcessis*. Moreover, he rejects all of the rationalists' ultimate premises. In the Introduction, he remarks on the way in which first truths have been smuggled in:

> 'Tis easy for one of judgment and learning, to perceive the weak foundation even of those systems, which have obtained the greatest credit, and have carried their pretensions highest to accurate and profound reasoning. Principles taken upon trust; consequences lamely deduced from them . . . . these are everywhere to be met with in the systems of the most eminent philosophers, and seem to have drawn disgrace upon philosophy itself.[1]

Hume uses his denial of abstract and innate knowledge to prove that all ideas come from impressions, that is, that experience is the *sole* source of knowledge. Hence there can be no a priori primitive propositions upon which can be based a deductive system that deals with the external world. This is his fundamental empiricism.

Now if Hume is to use the deductive method at all, even to disprove the contentions of his predecessors, he must have some "self-evident truths," expressed or unexpressed, from which to argue. The chief of these is Berkeley's axiom that the existence of a thing (idea) can only be proved by its perception or experience. This principle is rarely stated explicitly; Hume comes near to stating it when he asserts that the idea of the existence of an object is the same as the idea of the perception of the object.[1] He uses this principle to disprove the axiom of universal causation—no necessary connection can be found in the analysis of the experience of cause and effect, merely contiguity and succession, *ergo* there is no necessary connection between them.[2] He likewise uses it as the criterion of the existence of a soul—he finds no perception or experience of a soul, *ergo* there is no self or soul.[3] He also uses it to disprove any external world independent of experience;[4] in fact, it is fundamental to all of Hume's most important arguments. He may be said to have merely carried out Berkeley's major premise consistently and universally. Then we may conclude that, if Berkeley's major premise can be disproved and another put into its place, all of Hume's skeptical disproof of causality, the self, and the external world would fall to the ground!

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[1] *Ibid.*, Introduction, p. xvii.

Another of Hume's fundamental "self-evident" premises is "that all ideas, which are different, are separable."[5] He uses this as his major premise in dealing with the general problem of causality, by inferring from it that the ideas of cause and effect are different, hence separable, and therefore it is conceivable that a thing should come into existence (be an effect) without a cause. Conceivability implies possibility; hence universal causation has no intuitive certainty.[6] This principle of the "distinction of reason" is also used in many other places.

Thus Hume had his "self-evident" major premises which he did not demonstrate any more than did the rationalists. But he did not use them for constructive purposes; they were only and could only be used for the destructive demonstration of the positions that he desired to attack. Hume found himself using the deductive method of reasoning, and asserting that it alone could give knowledge,[1] but without any ultimate premises to use in deduction. The conclusion was inevitable, that there is no knowledge.

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[1] *Ibid.*, Bk. I, Part II, sec. vi, p. 66.

[2] *Ibid.*, Bk. I, Part III, sec. ii.

[3] *Ibid.*, Bk. I, Part IV, sec. vi.

[4] *Ibid.*, Bk. I, Part IV, sec. ii.

[5] *Ibid.*, Bk. I, Part I, sec. vii, p. 24.

[6] *Ibid.*, Bk. I, Part III, sec. iii.

Hume's criticism did away with rationalistic deduction from a priori principles. In its place he put another logical method, the description of immediate experience. Descartes had rejected this method when he contrasted sense-perception unfavorably with reason; but the Cartesian *cogito ergo sum* inevitably emphasized immediate experience, and Hume took such experience as the only certainty. "The only existences, of which we are certain, are perceptions"[2]; therefore if we do not perceive (have an impression of) anything, we cannot know it. This is Berkeley's axiom and Hume's fundamental critical assumption. Immediate experience cannot tell us anything with certainty regarding the external world; every such conclusion must assume the principle of the uniformity of nature, which cannot be demonstrated by any immediate experience. Therefore he concludes that "all knowledge resolves itself into probability."[3] Hume's logical method could not, therefore, lead to anything constructive—indeed, he has no right to assert anything more than a "bare probability" for his logical canons, and he admits that the most probable statement is only a probable probability, and so inevitably leads to infinite doubt.[4]

Hume's criticism was an attack upon all deduction, as he saw when he proposed to eliminate the rationalistic logic and substitute his own empirical logic. Deduction is going from the known to the unknown, i.e., from 5,763+8,492 to the unknown sum of these numbers, or from my perception of one side of a door to the other side. Hume saw the weakness of his method of empirical induction and did not know of any other developed inductive method; consequently he had only the method of immediate description left, whereby to acquire any certainty. When he used the method of immediate description, he could find no intuition for any of the major assumptions of deduction, such as "Every event has a cause." Likewise Hume could find no certain proof for any of the other assumptions of deduction, such as that where there is one side of a door there is another—when it is a matter, not of the definition of a door, i.e., not of the sort of ideas I have associated together, but of the sort of experience I can expect. Past experience can tell nothing of the future without the aid of certain knowledge; *ergo*, even if I perceive one side of a door now, I have no right to infer beyond my present experience, to the statement that there is another side of the door. When immediate experience is the sole way to truth, it is not surprising that positivism should have been the result.

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[1] *Ibid.*, Bk. I, Part III, sec. iv, last paragraph, where he equates "knowledge" with "scientific reasoning"; and *ibid.*, sec. xi, p. 124, where knowledge is defined as "assurance arising from the comparison of ideas," i.e., deductive proof.

[2] *Ibid.*, Bk. I, Part IV., sec. ii, p. 212.

[3] *Ibid.*, Bk. I, Part IV, sec. i, p. 181.

[4] *Ibid.*, Bk. I, Part IV, sec. i, p. 182; cf. § 28 of this chapter.

When Hume's argument is stated thus baldly, it is seen that he had no basis for any sort of knowledge beyond direct acquaintance and description of experience. Had he been content to present his criticism in this manner, his views would probably have been rejected, for we continually do infer from the present to the future, and our inferences are usually successful. Hume makes allowance for this fact by accepting a probability for inductive inference, although he has no logical right to assert that probability. This uncritical doctrine of probability adds tremendously to the persuasiveness of Hume's argument.

Mathematics, which was universally supposed to have necessary certainty, presented another problem. Hume was not a mathematician and did not feel able to attack mathematics, so he made a distinction between two sorts of relations, those "that depend not upon the mere ideas," i.e., relations of time and place, identity, and causation, and "those which are the foundations of science," i.e., resemblance, contrariety, degrees in quality, and proportions in quantity and number. The first group, Kant's synthetic relations, appear to give knowledge of the external world, and are all shown to fail to produce certainty in the disproof of the causal relation; but the second group of relations, which Kant called analytic, remain intuitively or demonstrably certain, although they do not relate to any objective world. Hence mathematics can have a sure foundation in these analytic relations, and can give "perfect exactness and certainty," undisturbed by Hume's attack upon the synthetic relations. Thus Hume guarded himself from any attack upon his denial of the possibility of knowledge which should base itself upon the view that mathematics does give certainty. There was one curious consequence of this distinction between these two sorts of relations. Geometry seems to relate too palpably to an external world; its axioms seem plainly to be drawn from experience, consequently Hume asserts that geometry cannot be an exact science, but can only give probability!

In distinguishing his two classes of relations, Hume failed to draw the full consequences of his fundamental principles. T. H. Green has shown that there is no real distinction between these two classes, and that the disproof of one class of relations applies equally to the other. Indeed, it is hard to see how it could be otherwise. With demonstration alone giving knowledge, and with a refusal to accept any positive ultimate premises, the only logical conclusion is that knowledge is utterly impossible, in mathematics as well as in physics. But Hume was living in a world which was too palpably gaining new insights into nature, and which used mathematics in that process. He was not ready to challenge Isaac Newton, who was the "master of them that know" in England. Hence he was unwilling at all times to follow his premises to their utterly skeptical conclusion.

§ 19. Immanuel Kant saw clearly that Hume's argument made metaphysics and philosophy impossible. There were only two alternatives, either to find new and seemingly unshakable ultimate premises or to discover a new method of proof and make it respectable for philosophy. Kant did both.

In the *Critique of Pure Reason* his fundamental problem is to validate the ultimate premises which Hume's attack had challenged, in order that thereby science (deductive science, both natural science and metaphysics) and knowledge shall be possible. Kant saw that while Hume had attacked only the principle of causality, yet the same sort of attack would invalidate all ultimate principles, so he generalized the problem raised by Hume, to include all the principles of knowledge. These must be firmly and unshakably established before knowledge can be possible by deduction from them.

In order to secure this unshakable foundation for first truths, Kant, like Locke, turned from logic to psychology, and discovered the truths he needed in the constitution of the transcendental sensibility and understanding. While Kant (and later logicians) did not call his procedure psychological, yet any discipline that attempts to describe what occurs in the mind during the process of knowledge (or perception) is fundamentally psychological in its nature, and such a description was exactly what Kant attempted. He asserted that the transcendental forms of the sensibility and the categories of the understanding modify the material of experience, so that experience invariably conforms to these forms and categories, and consequently the ultimate premises of mathematics, natural science, and immanent metaphysics will invariably remain true of all possible experience. Thus a priori knowledge is possible. These ultimate premises of deduction depend upon the transcendental operations of the mind (or the imagination), and thereby are kept always true. This is the meaning of Kant's Copernican revolution, which assumes "that objects must conform to our mode of cognition."[1]

What is the proof that Kant offers for the existence of these forms and categories? One ultimate foundation of this proof, as given in the most "critical" form of the transcendental deduction, as in other deductions, is Kant's recognition that experience, as it is apprehended, contains the relations of time, space, cause and effect, etc. Experience is not of unrelated, distinct, and inseparable ideas, as Hume had assumed, but it includes the experience of relations as well as of ideas. Experience is of objects, and an object is a complex of relations. Experience is united in the unity of self-consciousness. Thus relations are found in experience itself, not, as Hume assumed, added to experience, and our perceptions may not "be considered as separately existent."[2]

Nothing can be inferred from one premise alone. In contrast to this experience that contains relations within itself, Kant set the Humean atomistic unrelated conception of experience, which he called the manifold of sensuous intuition, the given, empirical, contingent, manifold material of sense. Kant conceived of it as a quantitative numericity of phenomena unrelated to one another. This conception of the manifold is inseparable from the Kantian argument, for without it there could be no proof of the necessary existence of the relating categories—experience would merely be apprehended as related and there could be no certain proof of the action of any relating categories. The existence of this manifold of intuition cannot be proved by Kant, for it is not experienced (at least, not according to Kant's "critical" view); he merely assumes it. It is part of his heritage from empiricism.

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[1] Preface to the second edition of the *Critique of Pure Reason*, p. xvi (of the German); p. 693 of M. Müller's translation.

[2] Hume, *Treatise*, Bk. I, Part IV, sec. v, p. 233. Cf. also p. 245.

Given thus the unrelated manifold of sensuous intuition and, given experience as related, what are the *conditions* that are necessary in order to produce experience with all its relations? Evidently what is necessary is a set of categories which produce these relations in experience as it is apprehended.

> We cannot represent to ourselves anything as connected in the object, without having previously connected it ourselves, and . . . . of all representations, *connection* is the only one which *cannot be given through the objects*, but must be carried out by the subject itself, because it is an act of its spontaneity.[1]

So Kant goes on to state in detail the categories he conceives necessary for the organization of experience, which include the forms of sensibility, the transcendental unity of apprehension, as well as the categories as modified by time to produce the schemata. This proof is summed up in the sentence, "The highest principle of all synthetical judgments is therefore this, that every object is subject to the necessary conditions of a synthetical unity of the manifold of intuition in a possible experience.[2] The manifold of intuition is unconnected, whereas experience is a synthetical unity; hence the relating categories are necessary.

This synthetic, relating process is not open to our observation (at least, not according to Kant's "critical" view), but can only be inferred from the presence of relations in experience as apprehended.

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[1] *Critique of Pure Reason*, second edition, "Transcendental Deduction," sec. 15; p. 744 of the M. Müller translation. Italics mine.

[2] *Critique of Pure Reason*, p. 158 of the German first edition.

> Synthesis in general is the mere result of what I call the faculty of imagination, a blind but indispensable function of the soul, without which we should have no knowledge whatsoever, but of the existence of which we are *scarcely conscious*.[1]
>
> This schematism of our understanding applied to phenomena and their mere form is an art *hidden in the depths of the human soul*, the true secrets of which we shall hardly ever be able to guess and reveal.[2]

Then inference alone can assure us of the existence of these categories. Kant is going beyond experience in asserting his categories!

The consequence of such a grounding of the ultimate premises of deduction in the constitution of human faculties is that deduction from such ultimate premises can only give sure results when confined within experience, so that metaphysical knowledge is restricted to human experience; transcendent knowledge is impossible. But these ultimate principles do not contain within themselves any limitation. They claim universality, so reason is constantly urged on to apply them universally. It therefore deduces conclusions concerning what is beyond experience, whence there arises the transcendental illusion that has been the source of the errors of metaphysics. Thus we see that the fundamental characteristics of the Kantian metaphysic arise from the attempt to validate the ultimate premises for deductive knowledge by founding them upon a psychological analysis.

It is easy to see the fallacies that were inevitable for Kant in such an attempt. He tried to establish the principles of logical deduction by *inference* from the psychological characteristics of the mind. But how could these psychological characteristics (the categories) be established? Evidently, only by a logical process, for they are *not open to observation*, as Kant observed. The very process of inferring from these psychological characteristics (the categories) to the ultimate premises of deduction is itself a process of inference. Thus the laws of inference are assumed before they can be established—the Kantian deduction involves a complete circle. This circle may be seen by considering what it is that makes *necessary* the deduction of the categories from Kant's two fundamental premises, the unrelated manifold and related experience. Evidently it can only be the axiom of causality—this change from unrelatedness to relatedness must have a cause, hence there must be the categories, including the category of causality! Kant's refutation of Hume, when logically analyzed as what he calls it, a deduction, becomes a mere *petitio!* Such a circular argument is inevitable in any attempt to derive logical principles from psychological characteristics or laws, or to infer them from anything else whatsoever, because these psychological or other laws can only be validated by a logical process, so that the laws of logic must be assumed before they can be derived!

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[1] *Ibid.*, p. 78 of the German first edition. Italics mine.

[2] *Ibid.*, p. 141 of the German first edition. Italics mine.

Kant's other great fallacy is the gratuitous assumption of the manifold, for which he could have no evidence, since it cannot be experienced, yet is needed for his argument.

While Kant called his proof of the categories a deduction (no other sort of proof would have been respectable in the philosophical circles of the day), yet it is not really deductive in character, as N. K. Smith has observed.[1] Kant had studied Newton, and so attempted to follow Newton's method of reasoning. Kant was seeking those conditions that are indispensably necessary for the production (causation) of a unified, related experience. These he found in the categories, etc., just as the astronomer finds the various orbits of the planets and the laws of their motion indispensably necessary for the production of the appearances of the planets in the sky. Having postulated these categories, etc., Kant proceeded to verify their existence

[1] *Commentary to Kant's Critique of Pure Reason*, p. 239.

by deducing synthetical principles of the pure understanding, i.e., the axioms of intuition, the anticipations of perception, and the analogies of experience, just as the astronomer verifies his hypothesis by deducing from his orbits and laws the phenomena of the heavens. Thus Kant's method of proof, while purporting to be deductive, is really inductive. As an inductive proof, Kant's argument ceases to be circular; he does not *assume* the axiom of causality to establish the category of causation, he merely postulates it.

However, even as an inductive proof, Kant's proof is still faulty. It shows the characteristics of an *ad hoc* hypothesis, in that it can account only for those facts which serve to shape the hypothesis itself; and it purports to establish the principles of logical method by a non-logical hypothesis, whose proof assumes logical principles. Kant's transcendental method is thus seen to be a founding of logical principles, which are established by induction from experience, upon a psychological analysis. For logic, the method of transcendental deduction is that of hypothetical induction. It is not surprising that the author of the nebular hypothesis should have resorted to induction.

The inductive character of Kant's method of proof was masked from the philosophic world of his time because he called it a "deduction." In his ethical philosophy, this sort of inductive proof becomes much more important. When he comes to state the categorical imperative, he does not deduce it—he merely states it; takes it, as it were, right out of the air, just as a scientist takes a hypothesis; then Kant justifies it by showing that, as stated, it conforms to the requirements of a categorical imperative, which he has previously laid down, and that its use leads to a valid code of ethics. "And although, no doubt, common men do not conceive it in such an abstract and universal form, yet they always have it really before their eyes, and use it as the standard of their deduction."[1] Of course, Kant has to use more than just the bare categorical imperative in order to deduce the ordinary principles of morality—he has often been criticized for assuming more items than his pure practical law, such as the existence of a social community of rational beings, etc. Deduction from one proposition alone is impossible; at least two are needed. Thus Kant, when confronted with the bankruptcy of a metaphysic that had founded itself upon deductive proof, turned to the inductive method of hypothesis. Kant is the central figure in modern philosophy, not only because of the wealth of his thought, but because his writings mark a fundamental change in the *method* of knowledge for philosophy.

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[1] *Metaphysic of Morals*, p. 24; Abbott's translation, p. 20.

Of course, Kant was not the first to use the inductive method among modern philosophers. Locke had used it a great deal; other philosophers had used it: in fact we shall see that its use is fundamental to all reasoning, deductive as well as non-deductive. But Kant was the first to put this method in the forefront of his philosophy and to make it the fundamental method of establishing essential positions. Kant's ethical philosophy was the source of later German philosophy, so we find the inductive method used by his successors. Methods of reasoning in philosophy, as in natural science, are propagated by imitation, not always by conscious analysis and deliberate cultivation.

Nevertheless, the true nature of Kant's reasoning was not recognized, either by himself or by his successors. One of the peculiarities of the inductive method is that, when its nature is not recognized, it appears to be a dogmatic assertion of the truth of a theory, followed by a deduction of its consequences. This characteristic of Kant's method appeared in the charge of dogmatism made against him by his contemporaries, and in his own dogmatism in his declining years. The inductive method has this difficulty—its use demands constant care and safeguarding; the consequences of each hypothesis must be carefully deduced and compared with the experienced facts; unless care is used, uncritical imitation of inductive reasoning results in untrammeled and often dogmatic speculation, in the propounding of vast hypotheses without any attempt at verification; and, instead of following a careful procedure, philosophy degenerates into mere speculation, in which a theory is only prized because it is new and strange. Because of the failure of Kant's successors to recognize the true nature of his method, and to apply the necessary safeguards, there eventuated this degeneration of philosophy into dogmatic speculation.

Kant, by his use of the inductive method as the means of establishing the categories, broke the hold that deduction, conceived as a method similar to that of mathematics, had held upon the thinkers of the eighteenth century. But Kant expressly called his method a *deduction;* he asserted a deduction of the categories, not an induction. The result of such a mistake was that Kant's successors, imitating Kant's procedure, used the inductive method but conceived of it as deductive, and so only half understood the procedure they were using.

§ 20. Fichte opens the *Grundlage* with a discussion of the method of reaching certainty or truth. He sees that the mere creation of a system of coherent propositions does not constitute a science, since that method would allow a hypothetical natural history of spirits of the air to be a science; a science must "assert something which is really known and can be known."[1] Mathematics is a science, i.e., constitutes knowledge, because it contains one proposition which is itself known, and which communicates its certainty to the remaining parts of the system.[2] Fichte conceives of this process of knowledge as deductive, and thinks of it as deduction from a single principle, as did Socrates in the *Republic;* consequently Fichte states that there can be only one proposition which is certain before a connection is made with others. The method of knowledge is to get one absolute proposition and deduce a system from that! Then the guaranty of correctness is that our fundamental principle is deduced from the system.[1] This last statement proves that Fichte really has the inductive method in mind, for an interpretation of hypothetical induction as deduction inevitably makes the process seem circular—the positions of a moving star in the sky are taken by astronomers as the basis upon which to build a hypothesis of its orbit, and that hypothesis is established because the positions the planet occupies in the sky are deducible from the hypothesis. Hence an attempt to *deduce* the orbit from the observed positions of the planet will show itself to be circular, since the observed positions will again be deducible from the orbit. Fichte notices this characteristic of his method and confesses its circularity with characteristic frankness. "Here, therefore, is a circle which the human mind can never get out of; and it is well to confess this circle plainly, lest its unexpected discovery at some time might confound men."[2] The circularity is only due to the fact that an inductive process is wrongly conceived of as deductive. Fichte likewise finds other circles, which are due to this misconception of his method.[3]

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[1] *The Science of Knowledge*, translation by A. E. Kroeger, p. 12.

[2] *Ibid.*, p. 13.

The first principle cannot be proved, says Fichte; "we have to *search for*" it.[4] It can therefore only be established by a non-deductive method. Once discovered, the world as a necessary connected whole of rational activities must flow from it. So Fichte invented a new form of deduction, the dialectical method of thesis, antithesis, and synthesis, in which the necessity that guides the process is not causal but teleological; when *D* is given, *A*, *B*, and *C* are necessary.

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[1] *Ibid.*, p. 35.

[2] *Ibid.*, p. 38.

[3] *Ibid.*, pp. 54, 59, 64, etc.

[4] *Ibid.*, p. 63. Italics mine.

From any empirical piece of knowledge, he abstracts the proposition, "*A* is *A*," finds that in order for *A* to be connected with *A*, it is necessary to postulate the identity, "Ego is ego, or I am I," which is necessary because it is "the ground of explanation of all facts of empirical consciousness." Likewise he finds the certain proposition, "$-A$ is the opposite to *A*"; hence among the acts of the ego there is an oppositing of the non-ego to the ego. Thus far, Fichte's method is the same as that of Kant's transcendental "deduction" of the categories—a process of searching for what is necessary in order that this knowledge may be—which is, as we have previously pointed out, an inductive process, although not recognized as such.

Fichte now proceeds to call in the process of dialectic to help him; having obtained thesis and antithesis, they must be got together. So the synthesis is produced, "The Ego opposites in the Ego a divisible Non-Ego to a divisible Ego,"[1] as the only way in which the thesis and antithesis can be combined without canceling each other. Now we have reached the level of experience, the limited empirical ego which opposes itself to the world of objects and limitations. Thus Fichte has justified his hypothesis by showing that it leads back to the world of experience. He then proceeds to deduce the universal forms of experience (the Kantian forms and categories) from these fundamental principles. Fichte himself recognized that the ultimate proof of his system lay in its success: "Whether such a system and its condition, a first principle, exist, cannot be decided in advance of the investigation. . . . . Everything depends upon the attempt."[2]

In his later works Fichte abandoned this scholastic method of proof, but the proof that he offers is still inductive. This

[1] *Ibid.*, p. 84.

[2] *Ibid.*, p. 30.

characteristic of his method appears beautifully in Miss Thompson's summary of Fichte's reasoning, in spite of her statement that he "deduced" his results.

> Fichte, through empirical observation and [inductive] reasoning founded upon this observation, reached the conclusion that the ego in its true nature is a free will that wills its own actual existence as the world of consciousness. But in examining the world of consciousness, he found everywhere law which the actual ego cannot evade. . . . . The only rational solution of these two contradictory views of the ego is . . . . the [inductive] assumption that a free will . . . . wills . . . . to appear as law. . . . . Hereupon the phenomenal world, the totality of consciousness, becomes visible [i.e., Fichte's hypothesis explains the apparent facts of the phenomenal world].[1]

This fundamentally inductive character also shows itself in the opening paragraph of Fichte's *Outlines of the Doctrine of Knowledge*, where he says that the problem of the doctrine of knowledge is, given knowledge as existing (the empirical basis of his theory), Upon what hypothesis can the existence of this knowledge be explained?[2]

Fichte's whole system thus becomes a magnificent hypothesis, whose cogency consists in its power to account for the facts of empirical consciousness and knowledge. The inductive character of Fichte's work has also been recognized by other logicians. Fichte's "construction was . . . . an ideal experiment by which a whole was seen to build itself up. The same is true . . . . of Hegel's Logic."[3]

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[1] A. B. Thompson, *The Unity of Fichte's Doctrine of Knowledge*, p. 17. The words within brackets are mine.

[2] "The Doctrine of Knowledge, apart from all special and definite *knowing*, proceeds immediately upon knowledge itself, in the essential unity in which it recognizes Knowledge as existing; and it raises this question in the first place:—How this Knowledge can come into being, and what is it in its inward and essential Nature?" (*The Popular Works of J. G. Fichte*, translation by William Smith, II, 501).

[3] B. Bosanquet, *Implication and Linear Inference*, p. 109.

The result of this unrecognized use of the inductive method by Kant and Fichte was that speculation was raised to respectability, since speculation is merely the suggestion of appropriate hypotheses with little attempt to verify them. The romantic movement helped to make speculation, which had now become philosophically respectable, the popular and almost universal activity of philosophers. Theories of the thing-in-itself, the absolute idea, the absolute will, etc., of the most diverse sorts appeared. Verification was neglected; the more different and the newer a speculation was, the better it seemed. *Schwärmerei* came into its own as a respectable method of philosophizing, with a freedom that would have shocked the eighteenth-century philosophers, who carefully reasoned and deduced their theories.

§ 21. The most elaborately worked out of these romantic theories is that of Hegel. His whole system is, logically considered, a vast hypothesis, which attempts to include, in its multitudinous details, all of the achievements of mind, past, present, and future—the most ambitious of all philosophic theories. Its procedure is to develop into one all-inclusive system the "inner activity and self-movement" of the life of thought itself. Consequently Hegel can only state what is the procedure of thought, and leave his system to be accepted or rejected as the reader sees fit. Only if the student makes himself "think in the form of the notion" and perceives "the immanent rhythm of conceptual thought" will he see that Hegel's statement is correct.[1] "The inmost truth of a philosophical work" is best "expressed . . . . in . . . . its results."[2]

The appeal of Hegel's system, so incomprehensible to some today, lay precisely in the magnificent extent to which it did appear to include the whole of knowledge in its grasp. The very bulk of the system and the extraordinary degree of insight it contained, impressed men, and made Hegel, the synthesizer of all human knowledge, the intellectual dictator of Europe. But the very size of his synthesis likewise brought about its own downfall—as soon as men found that Hegel, just because he had deduced by dialectic what the science of his own day believed true, had deduced falsehood, the hollowness of the dialectic method of deduction became apparent, and Hegel's system fell like a house of cards. The crucial exception to his hypothesis had appeared, and it destroyed his system.

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[1] Hegel, *The Phenomenology of Mind*, translation by J. G. Baillie, pp. 55–57.

[2] *Ibid.*, p. 3.

§ 22. Another and even more apparent use of the speculative method is the system of Schopenhauer, the "last of the great original speculative philosophers after Kant."[1] For him, the true reality is the will, an entity which is, in itself, not to be found in experience, but which expresses itself throughout experience. Because of the neglect which his own age accorded his philosophy, Schopenhauer was driven to attempt to verify his speculation, and in his later works he catches at every phenomenon, popular saying, or consideration that would tend to substantiate his hypothesis. There are a hundred gates to his system, he thinks, in that his hypothesis of the Will as the sole reality of things is exemplified everywhere. On the other hand, just as, to the scientist, his statement of a law becomes an *aperçu,* so to Schopenhauer the way to truth is by intuitive perception of it. Schopenhauer both recognizes the intuitive perception of truth and attempts an inductive proof for truth. He holds that the facts of observation are accounted for by only one hypothesis—that the Will is the sole reality of the world.

§ 23. It would hardly be worth our while to speak in detail of the other philosophies of that period or of the times since Hegel, since nothing essentially new in method has appeared.

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[1] W. Caldwell, *Schopenhauer's System,* p. 17.

We can merely note some of the tendencies which have been prominent. The downfall of Hegel's vast system made some thinkers more careful, and we find a renewed emphasis upon careful deduction or "transcendental deduction." The deductive method received a renewed impulse from mathematics through the discovery of logistic, symbolic or mathematical logic, and its cultivation by philosophers. On the other hand, speculation continued to be popular, and we find speculations as unrestrained as any of the romantic period. Theories are esteemed because of their *novelty;* the philosophic genius comes to be thought of as the man who can think of something *new* and previously unthought of, the imaginative poet of the adventures of the mind. Were philosophy a collection of fairy stories, or were it valued by the same standards as poetry, such an appeal to imaginative novelty could be tolerated; but philosophy professes to deal with truth, and even the wildest speculations *profess* to be true. Unless the philosopher gives up all pretensions to the truth, he ought to restrain his imagination by serious and careful checks. Surely truth is not a fashion which changes with the winds of opinion and novelty, and the prosaic and hard-won truths of the ancients are not to give way to a view whose only recommendation is its novelty. In this world of ours, truth at least appears to be unchanging, and so the presumption is that any opinion which has been accepted for ages should contain far more truth than a new-blown speculation.

§ 24. We likewise find speculations valued for their *inclusiveness*. The more extended the ground which a speculation covers, the better. It is natural that this tradition should have attached itself to Hegel, the all-inclusive philosopher. This criterion is the coherence theory of truth, whose defects we have previously pointed out. Surely the failure of Hegel's all-inclusive system should have taught men that all-inclusiveness is just as likely to bring about the downfall of a theory as lack of inclusiveness is. Until human intelligence succeeds in literally comprehending the Absolute All and in stating it so that we can understand all its details with nothing remaining unknown, mere comprehensiveness will remain worthless as a criterion of truth; for the discovery of one unreconcilable fact may bring about the downfall of the whole structure, just as Hegel's construction collapsed before the impact of new discoveries.

§ 25. Another common procedure has been to make an *analysis* or *description* of certain parts of experience and present it as truth. Most of the idealistic and pragmatic logic of the day depends for its cogency upon this method of description and analysis. The description is merely presented; it is justified by no logical process, and the fact that these statements are found by analysis or description is taken *ipso facto* as a criterion of their truth. This method, like so many others, is historically derived from Kant's transcendental method, in which he attempted to lay the foundations for logical procedure upon a description and analysis of the nature of knowledge.

The description or analysis is not itself the experience that is described. It is clearly other than the experience; when a description refers itself to the experience which it describes, it is clearly *transcending* itself. Under these circumstances it is natural that some philosophers should object to all description as falsification. In so far as no description, even the best, can *be* the experience which is described, that objection has weight; but the consistent carrying out of the position that all description is falsification would lead to the abolition of all philosophy and all reflection. Even the critic's description of the position he is attacking cannot be that position, and hence would be falsification of that position; therefore he can only refute a position which he first falsifies! Knowledge, if it is *knowledge* *about* anything, is transcending itself; any objection to such transcendence is objection to all sorts of reflective knowledge, including memory knowledge, and leaves as valid only the experience of the moment.

A description or analysis, which thus transcends itself, needs justification. If it were possible to make only one analysis of a given fact or situation, description or analysis would be a good guide to truth; but unfortunately we find that different individuals make different descriptions and analyses of the same thing or the same event. Modern scientists, especially in the realm of psychology, where the description of experience is especially difficult and easily influenced by preconceptions, have learned the importance of the logical justification of an analysis or description. Logically considered, the description or analysis becomes a hypothesis, which depends for its verification upon how well it fits the experience that it describes. But most logicians have not considered truth to be obtainable by the "method of hypothesis," and so have left the metaphysical and logical foundations of their theories resting upon mere descriptions or analyses which themselves have no logical justification except that the reader may arbitrarily accept them as true.

§ 26. The most important event for philosophy in the last century was the discovery of non-Euclidean geometry. Mathematics has always influenced philosophy profoundly; that influence has been chiefly due to the fact that it was the most developed of sciences, so that its influence has waned since the development of other sciences. Mathematics has been to philosophers the outstanding example of absolute and certain knowledge, and geometry has traditionally been the showpiece of mathematics. It has been the outstanding example of a well worked out deductive system, and was thought to be founded on eternally self-evident truths. But now mathematicians themselves have destroyed the certainty of the foundations of geometry—they have shown that mutually incompatible suppositions may be made and developed into self-consistent geometries. Thus consistency has ceased to be an undoubted criterion of knowledge, and mathematics has come to be, not eternal truth, but merely the consistent development of a set of postulates. These postulates may themselves be true or false—mathematics itself cannot tell which—and other incompatible postulates may result in the development of other systems different but equally self-consistent. Thus mathematics has ceased to claim truth for itself, and its development, while it may be an interesting game, is no longer conceived to be the discovery of eternally necessary truths. The last bulwark of rationalism has gone, and the "contemplation of the eternal truths of mathematics" as a solace for the disappointments of life, which satisfied a Spinoza, has become a contemplation of a set of propositions that may be conceived thus, but may equally be conceived otherwise, depending upon whether one or another set of postulates is accepted. When a philosopher uses the deductive method of proof today, he can no longer claim evidential value for it, but can merely say that he is presenting one possible way of looking at things.

§ 27. The pragmatic movement, which has been so influential, is, methodologically considered, an appeal, more or less explicit, to the inductive method, but without a careful criticism of that method. A statement is true because it "works," as for example, mathematics is true because it works—what could be more ambiguous than the phrase, "because it works"? The pragmatists have done yeoman service in attacking and criticizing "absolutism," i.e., the purely deductive method; but they have failed to criticize their own method adequately. Pragmatism represents, today, a revolt against traditional authority and the traditional methods; but it has failed to present any new method in place of the old except the appeal to "working." The plausibility of this appeal is that it seems to fit in with the method of natural science and of the practical man in his acceptance or rejection of his beliefs. The use of the inductive method is universal; it is, perhaps, the oldest and most widely spread of methods of justifying the truth, so the pragmatic appeal to "working" has had a wide acceptance. Nevertheless the man on the street recognizes that falsehoods work as well as truths; the scientist recognizes that although the Ptolemaic theory is universally rejected and the Copernican theory is accepted, yet the "pragmatic difference" between them is slight. Just what interpretation should be given to the "workingness" of truth has not been settled; some pragmatists have denied that when a supposed truth satisfies human desires it should be considered as "working" and hence true; yet there is a continued and often justifiable suspicion that, just because of the essential ambiguity of the term, "working," the pragmatists have allowed personal desires to mold their statements as to the nature of the world. The unanswered problem for the pragmatist is this: What sorts of "working" shall be allowed as criteria of truth and what sorts shall be rejected? and How can you justify your acceptances and rejections?

The reason that pragmatism has allowed itself to be so vague regarding the criteria of truth is that it has accepted the dicta of Locke and Hume, that induction or the experimental method can only give probability.[1] Because the pragmatist has conceived that the experimental method can give only probability, he has allowed himself to be vague about it. He says to himself, After all, the results of experimental method are never certain, they can only be probable, that is, other things may also be true; so why worry about the details of a method that is itself faulty and unable to give certain truth? Pragmatism has thus allied itself to the skepticism that denies the possibility of any knowledge in the strict sense of the word.

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[1] Cf. chaps. vii and ix for a discussion of this assumption.

§ 28. Pragmatists, and some other philosophers, have often accepted the view of ancient skepticism that certainty is impossible, but that, nevertheless, probability can be had, so that life can be guided by probability. Such philosophers have failed to understand the ultimate consequences of their view that only probability can be attained. If probability means anything, it means that a merely probable conclusion is one which I am not forced to accept, but which has only claims to truth, no certainty. Other views may also be true, but *probability can never rule out the possible truth of other theories*. Yet these philosophers have gone on to attack other philosophies, and to assert that their opponents are wrong, forgetting that by their own confession, the falsity of the views of their opponents can never be shown, just as the truth of their own position cannot be evidenced! A philosopher who accepts the view that his logical method can only give probability can never logically assert that any opponent is mistaken, for probability can never rule out any opinion. Logically this man ought to accept all possible views, his opponents' as well as his own, as all possessing claims to acceptance, but none of them possessing any exclusive claim that logically *commands* acceptance. All thinking, accordingly, resolves itself into the perpetual balancing of diverse opinions, for knowledge does not exist.

To this attack, the reply would very likely be made that there are degrees of probability, and that we should accept the view which has the greatest degree of probability. The usual judgment of probability, e.g., that "this view is very probable," assumes that the statement of probability is itself certain, not probable. But the upholder of mere probability has no logical way of attaining any certainty about his statement of probabilities. He can only say that it is very probable that his view is very probable. That statement, again, can at best only be very probable, and thus he is committed to an infinite regress of probabilities, which ultimately reduces all probabilities to equal rank. A mathematical exposition will show that this conclusion is inescapable. To test the consequences of the theory that only probability is attainable, let us assume the correctness of the procedure of mathematical deduction; then let us also assume that our original probability can be expressed in mathematical terms, say 99 per cent, and that the succeeding probabilities of probabilities, etc., can also be expressed in mathematical terms, all of which are equally high, 99 per cent. Then the probability of the probability of the probability, etc., of the original probability would be expressed by the infinite product, 0.99^{∞}. But $\lim_{n=\infty} 0.99^n = 0$. The limit of an infinite series of probabilities of probabilities, all members of which are less than 1.00, is 0, and all infinite series of probabilities of probabilities, have the same limit, 0. The same result would be reached, of course, if the probability could not be stated in numerical terms. Hume similarly recognized that his denial of any certainty reduced any probability, no matter how great, to infinite doubt.[1] Thus while no single probability of a probability, etc., can actually become an impossibility, yet, *unless somewhere certainty can be found* to stop the infinite series of probabilities, *all probabilities reduce to infinitely small quantities*, independently of each other, and consequently no one philosophical theory can have any preference over any other.

Those logicians who accept the view that probability alone can be attained have, however, failed to draw this conclusion from their premises, unavoidable as it is. They have talked about "practical certainty" or "experimental certainty," which they hold is sufficient for them. The brave man needs no certainty; the practical certainty of life, which does not exclude the possibility of error or mistake, is sufficient for the tough-minded. Yet such philosophers do need that "practical certainty," which for them is a high degree of certainty. The skeptical pragmatist, for example, feels able to criticize other philosophies, and thinks that he has *disproved* them. He feels that "practical certainty" is sufficient to enable him to eliminate certain theories and substitute others. Thus the pragmatist's "practical certainty" does everything for him that the absolutist's "absolute certainty" does for the absolutist—it disproves the views of others, it gives him the feeling that he is right and others wrong—it does everything except remove the vague and purely theoretical possibility that he may be in error, a possibility that he leaves practically out of account. Thus there is no *pragmatic difference* between this "practical certainty" and the absolutist's "absolute certainty"! These philosophers forget that a proposition can eliminate its contrary or contradictory only when it is *true;* whereas, if two contraries or contradictories are merely probable, they do not destroy each other. Such logicians have assumed that their "probability" is a *certain* probability; they have failed to see that if no certainty is obtainable at all, their views can only have a probable probability, whose probable probability is only probable, and that the total probability of all merely probable, probable, etc., probabilities is infinitely small, so that all theories become equally true or false, and there is no basis for attacking any opponents or adhering to any theory of one's own.

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[1] *Treatise*, Bk. I, Part IV, sec. i, p. 182.

§ 29. While philosophy thus began by using the hypothetical inductive method, yet the influence of mathematics very soon turned it to the deductive method. This deductive method, however, created certain difficulties for ancient philosophy, so that its use brought about the prevalence of skepticism, mysticism, and dogmatism. Modern philosophy likewise began with the deductive method, until, in the hands of Hume, that method brought about the bankruptcy of philosophy. Kant surreptitiously introduced the inductive method, although without recognizing its character, and consequently without adequately safeguarding its correctness. Thereupon there ensued a burst of speculation and dogmatism. Today philosophy has at last recognized the inductive method, but at the same time it has accepted the belief, which came from Locke and Hume, that induction gives only probable results. Where, then, shall we look for knowledge? Is there any possibility of gaining permanent truth at all? How can a careful thinker avoid the conclusion that philosophy is not a science, that it cannot hope for knowledge, and that, since no logical method can give anything more than probability, no science (or knowledge) at all is possible, since all sciences must use the logical methods which philosophy analyzes? Skepticism seems ready to fasten its talons upon philosophy again, and a new period of stagnation to threaten.

Skepticism seems indeed to be in the air. Men point to the fact that the natural sciences obtain results which command the assent of all who are competent to form an opinion, while there is hardly an important philosophical theory, and certainly no philosophical system, from which other thinkers, no less competent, do not violently dissent. When we think of Kant's words, which we placed at the beginning of this part of the book, what other conclusion than the skeptical one is possible? Mr. Santayana tells us that the only way out of skepticism is by animal faith, which is essentially irrational in its nature; some philosophers today claim for their theories nothing more than the character of, at best, probable hypotheses, far inferior in their probability to the theories of science. The course of philosophy is held to be from certainty to doubt.[1] Yet in spite of many skeptical voices, it is doubtful that skepticism will really prevail. The skepticism of the ancient world was founded, not only upon the failure of philosophy, but also upon the failure of men to deal successfully with the material and social world. Our modern world is most of all different from the ancient world in just that respect. Never has natural science been so successful in changing the face of the world. It is hard to believe that these successes of natural science have been achieved without the use of knowledge; however much we may be skeptical of the truth-value of contemplative sensation or reflection, this success in *acting* upon and in remaking the world surely implies that certain knowledge has been attained. Let us at least make that assumption, and see if we cannot, in the sequel, justify it to ourselves by a more rigorous logical method.

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[1] R. F. A. Hoernle, *Studies in Contemporary Metaphysics*, p. 47.

PART II

THE THEORY OF RATIONAL INDUCTION

CHAPTER IV

THEORIES OF INDUCTION

> What induction is, therefore, and what conditions render it legitimate, cannot but be deemed the main question of the science of logic—the question which includes all others.[1]

§ 1. We have found that none of the logical methods enumerated by philosophy can be admitted to give knowledge. Induction is everywhere admitted only to give probability; deduction has shown itself to be the most helpless of all logical methods, because it can neither provide its own primitive premises, nor establish its own correctness as a method. Authoritarianism and mystical intuition were shown to be non-logical methods; the former was found to be utterly incapable of meeting criticism, or to depend for the justification of its authority upon some other logical method, and the latter was found to be incapable of logically justifying its discoveries to anyone except the original discoverer. We also saw that it is highly improbable that any different sort of method will be discovered. We traced the history of method in philosophy, and found that the deductive method was the only widely used method of exact proof until Kant invented the transcendental method of searching for the presuppositions of an admitted fact, which turned out to be inductive in its nature, although he called it a deduction.

Where then shall we turn? There seems to be but one possibility of escape from the difficulty—upon re-examination of our analysis of logical methods, we find that we have accepted the current opinion that induction can give only probability, but we have not ourselves carefully examined the nature of inductive methods. Might it be possible that logicians have been wrong in their assertion that induction gives only probability? We must, however, admit the correctness of their conclusion that the traditional methods of inductive logic, such as Aristotelian enumeration, Baconian induction, and Mill's canons, fail to give certainty. Nevertheless there remains the question: Do the traditional inductive methods of the logic books represent the real procedure of induction? Might it not be possible that logicians have misunderstood the nature of induction and so misjudged its worth? In this direction seems to be the only hope of escape from skepticism.

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[1] J. S. Mill, *Logic*, Bk. III, chap. i, sec. 1, p. 185.

§ 2. But what of current epistemological theories—idealism, realism, pragmatism? Upon examination we find that they all suffer from a defect which prevents them from being of any use to us, a defect which ultimately traces itself to a misunderstanding of the logical significance of Kant's transcendental method. These epistemological theories all base themselves upon an analysis of the nature of knowledge. The idealist finds that all knowledge consists of ideas, hence develops his epistemology and metaphysics in accordance with that characteristic of knowledge. The pragmatist finds that knowledge is instrumental in its nature, and founds his epistemology upon that fact. The realist makes an analysis, in physiological and psychological terms, of the relation between the mind and its object, then develops his realistic epistemology. The point which we must stress is that each of these theories is founded upon a logically antecedent analysis of the logical or psychological structure of knowledge. These analyses differ; which one is correct? While they might all be true, and their implications misconceived, yet it is evident that there must be some way of justifying an analysis; it would be foolish to accept every analysis that is offered. Surely these analyses are not merely dogmatic, but are justified by their proponents. Furthermore, if an analysis of *knowledge* is to be made, then there must be some logically antecedent method of identifying knowledge before the analysis is commenced. Each of these epistemological theories, therefore, *logically presupposes* a method of reaching and justifying knowledge before it can begin to establish its own theory! The analyses of the idealist, realist, or pragmatist may be correct enough, but if they are to be more than dogmatic statements, they must be *proved* to be correct, and it must be proved that we are given an analysis of *knowledge* and not something else; hence some method of proof or of attaining knowledge must be assumed before the idealistic, realistic, or pragmatic epistemology can be founded upon it.

We are in search of exactly such a method of justifying knowledge, and consequently must dismiss the current epistemological theories as themselves presupposing what we are searching for, although none of them have been able to enunciate such a theory. Against their claims we must reiterate the ancient statement that logic is prior to all knowledge, prior even to epistemology. If we can solve the logical problem of justifying knowledge, perhaps we shall find that the epistemological problem will have become easily solvable. We must separate these two problems; until logic has established a method of justifying knowledge, even an analysis of the nature of knowledge will be presumptuous.

§ 3. We must likewise turn away from any form of induction that uses the theory of probability as a means of, or as an aid in, induction. The theory of probability, like all other mathematical and logistic disciplines, is deductive in nature, and so is founded upon a set of primitive propositions which the theory cannot itself examine. Probability is always relative to some "facts" or to some "knowledge," hence is derived from those "facts" or that "knowledge" by a process which uses the principles of deductive logic in the derivation. We are seeking a method of evaluating such primitive propositions, and cannot use any deductive method which relies upon such propositions. How are we to obtain those "facts" or that "knowledge" upon which the statement of probability is founded? Any statement of a probability itself claims certainty: when, for example, I assert that the probability is $\frac{3}{10}$ that the next ball I draw out of the bag will be white, my statement itself claims to be true. Then any method of obtaining a statement of probability must itself be a means for obtaining certainties, not mere probabilities. The theory of probability itself claims certainty, and so must be founded upon certain and true primitive propositions. Unless there is some way of obtaining logical certainty, even the statement of probability which I obtain by the inductive method will itself only be probable, and that probability will again be only probable, so that every statement I make will have the logical worth of the product of an infinite series of probabilities, and consequently there will be no degrees of probability, but all probabilities will become infinitely small.[1] Unless certainty can be somehow obtained, there can be no degrees of probability, either mathematical or otherwise; we ought to be skeptical of all happenings. Certainty alone can enable us to make significant statements of any sort.

§ 4. Furthermore, we must refuse to use any method of induction that founds itself upon the process of enumeration of instances. Ever since Hume based his destructive analysis of the relation of cause and effect upon the assumption that the proof of that relation consists only in an enumeration of instances of constant conjunction, logicians have almost always fallen back upon the enumeration of instances as the ultimate justification for an induction.[2] Such an enumeration can, of course, lead only to a high degree of possibility at best, though some logicians have denied that by itself it gives even that.[1] If this were the only logical method of gaining truth, all knowledge would disappear; it would be reduced to mere probabilities that would become infinitely improbable.

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[1] Cf. chap. iii, § 28.

[2] J. M. Keynes, *A Treatise on Probability*, Part III, and C. D. Broad, "The Relation between Induction and Probability," in *Mind* (1918 and 1920), are

The fatal objection against enumeration of instances is that scientists do not use this method when another is available. We shall later see that those propositions which are proved solely by a process of enumeration (which we shall call *empirical* generalizations) are so different in their import from truly *rational* (or scientific) generalizations that they are different in kind.[2] The real reason why the intelligent man believes that the sun will rise tomorrow is not because it has risen yesterday or any number of yesterdays—such a statement of the reason for his belief is a gross caricature of the procedure of modern

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conspicuous examples. While Keynes disparages enumeration of instances, and admits that such instances can give only added probability when there is already a finite initial probability derived "from some other source in favor of the generalization and, assuming the generalization is false, a finite uncertainty as to its conclusion being satisfied by the next hitherto unexamined instance which satisfies its premise" (Part III, chap. xx, sec. 6), yet he has no satisfactory method of obtaining this necessary initial probability. It is based "partly on the *number* of conclusions, each seemingly true . . . . which can be derived from the axioms and partly on a degree of self-evidence in the axioms. . . . . If a proposition appears to us to be true, this is by itself, in the absence of opposing evidence, *some reason* for its *being* as well as appearing true. . . . . Unless we can assume some substantial relation of probability between the appearance and the reality of truth, the possibility of even probable knowledge is at an end" (chap. xx, sec. 8). Thus the subjective *appearance* of plausibility which a generalization has for us must be admitted to be the ultimate source of probability, and unless we can *initially assume* that appearances are at least slightly reliable, all knowledge is impossible! Surely such an admission can lead only to skepticism of the possibility of knowledge, unless some other ultimate method than that of probability or enumeration is available.

[1] Keynes, *op. cit.*, Part III, chap. xx, sec. 6.

[2] Cf. chap. vii and chap. viii, § 3.

science and reduces the scientist to the level of the savage; the intelligent man believes that the sun will rise tomorrow because the earth is an oblate spheroid which is turning on its axis and which is kept at a (roughly) constant distance from the sun by physical forces, and this physical and astronomical conception of the universe *implies* that the sun will rise tomorrow. For the man who has accepted this conception of the universe, the notion that the earth cannot stop turning on its axis without the intervention of some further condition becomes as certain as any mathematical equation. This scientific conception differs vitally from a statement of an enumeration which merely gives the probability of the same event happening again, in that it contains within itself a statement of the conditions upon which that event would *cease* to occur. In the case of the rising of the sun, this astronomical and physical conception of the scientific laws underlying that event does anticipate that, as a result of the friction of the tides, etc., the time will come when the sun will cease to rise, and the earth will present only one face to the sun, just as the moon now presents only one side to the earth. This scientific conception also implies that, if some large body should enter the solar system, the relations between the parts of the system would be greatly disturbed, and a radically different system would emerge, in which there might even be no earth for the sun to rise upon! The foregoing illustration shows what a gross misunderstanding of the true nature of scientific induction is the conception that induction consists ultimately of enumeration of instances. The result of enumeration can only at best be a statement of a (uncertain) probability that a particular event will be repeated; the result of scientific induction consists in the statement of scientific and rational, not empirical, laws, which contemplate even the cessation of the very event which is being investigated!

§ 5. There have been many other conceptions of induction which do not need more than a mention, since even their proponents admit that they do not produce that certainty which alone can save the verity of philosophy from being reduced to an infinitely small probability. Induction has been conceived as the elimination of alternatives,[1] as a syllogism with the propositions summing up the relevant observations as minors, and a major consisting of some universal proposition,[2] or as the search for ultimate premises from which empirical facts may be deduced,[3] or as a process of interpolation and extrapolation of hypothetical intermediate steps between the observed facts,[4] and in other ways. Each of these accounts of induction assumes the deductive method and establishes induction upon the basis of deduction. But we have seen that deduction itself needs to be validated, and that of all methods it least of all can establish itself.

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[1] H. W. B. Joseph, *Introduction to Logic*, especially chap. xx. Cf. B. Bosanquet, *Implication and Linear Inference*, chap. iv for criticism. Of course the greatest difficulty of the eliminating process is the establishment of an exhaustive disjunctive major premise. When all the possible alternatives are catalogued and it is proved that there can be no more alternatives, the rest of the work, the elimination of the untrue alternatives, is comparatively easy.

[2] Thomas Case in *Lectures on the Method of Science*, lect. i. A difficulty lies in the proof of the factual minor premise. Such a theory also assumes the logically helpless deductive method, and is unable to answer such ultimate questions as: What is the proof of the ultimate foundations upon which the universal major premise is established? How validate the deductive procedure?

[3] B. Russell, *Principles of Mathematics*, p. 441. But in view of the fact that our senses sometimes at least delude us, how are we going to be sure of our empirical fact? Is it to be simply accepted on authority, or are all appearances to be accepted uncritically as facts? This conception also leaves unanswered the problem of the validation of the deductive procedure. It assumes that it is possible to determine *all* possible sets of premises for any proposition, and that the number of sets is so small that elimination is possible. Since two premises are needed to produce an inferred conclusion, the process of tracing back a single empirical premise to its primitive propositions seems to promise to be impossibly complex.

[4] Cassirer, *Substance and Function*, p. 259. Are we to interpolate any and every intermediate step that happens to come to our minds, or are we to select from them? If so, on what basis are we to select certain interpolations as correct? Then interpolation and extrapolation do not throw any light upon the logical problem of discovering truth. Similar to this theory is the conception of Bosanquet, "You draw out a construction, supplying the relations necessary to make your subject intelligible, and you read off your conclusion from the result" (*Implication*, p. 114). But will *any* construction do? Certainly not in law or biology; only a construction which "fits," i.e., is true, will do. But how can we

§ 6. Perhaps the one conclusion we can draw from the struggles of logicians over induction is that it is useless to endeavor to sit down and excogitate logical methods out of one's own mind. Successful and useful logical methods have never been thus discovered. When Aristotle formulated the syllogism, he was not so much inventing a new method as formulating and codifying the methods of procedure that had been worked out in Athenian forensic. The logic of mathematics likewise underwent much trial and error, before it could persuade thinking men of its infallibility. The cause of Bacon's failure to state the correct procedure of science lay in his attempt to think out a method for himself before he applied it; his failure to recognize the value of the work of Gilbert, the greatest English scientist of his generation, merely illustrates his ignorance of the true nature of scientific procedure. It is useless to try to evolve out of our inner consciousness a criterion of truth. Such a procedure can only result in dogmatism; the value of the result depends upon the extent of our experience in discovering truths and verifying their correctness.

Consequently, it is methodologically preferable to give a description of the characteristics of the method of induction as they have been worked out in the process of criticizing discovered truth, rather than to excogitate an arbitrary statement of our own. We shall, however, attempt to state this method in such general terms as are common to all sciences. In stating it,

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tell whether it fits? Merely by its "fittingness"? Bosanquet has the rational inductive law in mind, but fails to analyze its nature carefully.

we can assume nothing, not even the categories of the deductive method. Consequently we can base this method upon nothing more ultimate than itself. If it is true, it must validate itself; it cannot be validated by anything more ultimate. For we are seeking for the most ultimate of all ultimates— the criterion of knowledge. It cannot be deduced from anything else; yet it must allow the application of itself to itself in order to validate itself. If men have ever attained any knowledge at all—and it is difficult to believe that knowledge has never been attained by men anywhere—it must have been grounded upon this method. Hence we ought to find it used everywhere, once we can recognize it, although it may have previously failed of recognition. If we analyze correctly the methods of proof used by those individuals who have been most conspicuously able to attain truth, we should be able to discover the method we need. Confirmation of the correctness of the conception we shall propose is to be found in the fact that this general method of induction was originally formulated as a logical theory by the first philosophic historian of science, a man who was not only thoroughly acquainted with the fields of philosophy and natural science, but who was also a scientist and himself interested in making scientific discoveries. Unfortunately Whewell put his statement into such a Baconian terminology and overlaid it with so much Kantian epistemological theory that it is necessary to restate his discovery and free it from extraneous matter.

§ 7. Before we start on this task, we must stop to consider more exactly what we want. Do we need a method of discovery or a method of proof? Bacon was led astray by his pursuit of the first of these ideals. Methods of discovery would be valuable, if they were attainable, but definite methods of this sort seem to be impossible. Each science has developed a set of guiding principles, but they do not pretend to be more than good advice about the method to pursue in investigations; some discoveries have been made in violation of these rules instead of by following them! Each particular science seems to have a different procedure, and there seems to be little common to them all, except an appeal to that most vague of psychological procedures, intuition. Even in such a deductive science as mathematics, advance in knowledge is most uncertain; it is dependent upon the intuition of those few individuals who are particularly fitted by temperament for such work. Discovery seems to be a psychological process of intuition that obeys no known laws. We may well agree with the historian of science, "Speaking with strictness, an art of discovery is not possible."[1]

§ 8. Discovery is important, but verification is even more important. It may take a genius to make discoveries, but if every self-styled genius were to be allowed to make all the "discoveries" he wishes, and no criticism were to be exercised upon them, science would be in a pretty mess! Even Newton, great genius that he was, discovered in his theory of optics at least as many errors as truths. We cannot rely upon the authority of even a Newton for truth. The task of criticism is much more important than the task of discovery. Furthermore, the task of logical criticism seems to be one that is not limited to the genius, but can be performed by the ordinary man, and can be reduced to rules.

We must then give up any hopes of attaining a method of discovery, and confine ourselves to the more humble and attainable task of seeking a method of criticism. Consequently we shall conceive logic, not as a discussion of modes of thought—that attitude leads into the field of psychology—not even as a consideration of methods of correct thought—even such a definition is likely to lead us astray into psychology—but as the criticism of any thought that pretends to be true or valid.

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[1] Whewell, *Novum organum renovatum*, p. 142.

Logic is the science of testing and proving propositions; its function is that of criticism, not discovery. Of course the process of testing propositions leads us to the classification and definition of propositions and of their parts as well as to the discussion of logical operations.

Logic should state a form of reasoning into which our thought, in so far as it relates to the truth or correctness of our conclusions, can be put, and which will indicate both the strong points of a good argument and the weak points of a faulty one. The Aristotelian syllogism does that for many deductive arguments; modern symbolic logic fits other cases, mathematical reasoning deals with a still different set of reasonings. In deduction we have a quite comprehensive set of trustworthy forms into which arguments may be thrown. Nevertheless, only a part of philosophic reasoning is deductive, and that often the smaller and less important part, for philosophy deals with ultimate and general truths, which cannot usually be established by deduction, just because they are ultimate and general. Hence philosophy, more than any other science, needs to investigate the requirements for the cogency of that ultimate reasoning which concerns itself with just those conclusions that cannot be established by deduction. Logic, in the past, has often been anything but that. It has been a study of the kinds of judgments and concepts—an analysis of the nature of knowledge, which is yet without a method of establishing the correctness of that analysis. All that such a logic can do is to make its analysis and then let others judge for themselves whether that analysis is well or ill made; there is no standard of proof except the persuasiveness of the argument or the judgment of the reader. The result has often been bad analysis and bad philosophy. Logic must explicitly set itself to the work of criticizing itself.

§ 9. Induction may be defined as the method of establishing the truth of propositions; not from those more general, but from those less general or from mere experiences which have not been stated in the form of propositions. Since our theory of induction cannot depend upon anything more ultimate, but must be justified by the application of itself to itself, we can only proceed to state our theory. The justification of that theory must be left to later chapters. In expounding our theory, we must be apparently dogmatic; we accordingly crave the tolerance of the reader and give him the assurance that we shall later show him that our dogmatism is necessary, but only apparent, and that in the argument of chapter x we shall attempt to make amends for our present apparently dogmatic procedure.

The thesis which we shall expound and later defend is that induction always concerns itself with the establishment of the correctness of a hypothesis or assumption. The attempt to establish this hypothesis may be by one of three ways: empirical induction, immediate description, or rational induction. Empirical induction is the familiar method of enumeration of instances, leading to the proof of an empirical generalization, which can, therefore, never be established with certainty. Immediate description is limited in its use to the rather uncommon situation in which an experience and its description (the hypothesis) are held in mind at the same time, and can therefore be compared. Rational[1] induction is the most important of inductive processes. It is the most general of them all; the other two are merely modifications or aspects of it. It is such a simple and natural psychological procedure that it has been used in all ages and by all men in testing their beliefs, especially by men of insight. Its very simplicity, however, has often led to its being misunderstood by those who have used it, especially by logicians, who have too often tried to assimilate it to the better-recognized deductive procedure. In its general characteristics it is very simple, but in its details it often becomes extremely complex.

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[1] We have styled it "rational," because (1) it is desired to contrast this method with the traditional *empirical* method, and (2) because this method uses as an important integral part of itself the traditional *ratio*—deductive inference.

This general procedure for the logical testing of a rational inductive generalization consists, first, in the formulation of a hypothesis; second, in the elaboration of the consequences of that hypothesis, which is deduction; third, in the checking of those consequences with experience, to see whether they are exactly found in experience, which is often by immediate description; and fourth, in the judgment as to whether the hypothesis has been verified (is true) or not. We shall consider these four steps in order.

CHAPTER V

THE HYPOTHESIS

§ 1. A hypothesis is any statement or set of statements that is considered (not dogmatically asserted) as a candidate for verification or for knowledge. It is by the aid of hypotheses that we endeavor to describe or explain our experiences, to state their orderliness, their inner nature, their causation, or some similar characteristic. The hypothesis differs from a deduced proposition in that it is not established by anything more general, although it may be derived from a more general supposition by deduction; it is, however, more often obtained by an intuition.

§ 2. The importance of the hypothesis in the method of discovery has been emphasized by many logicians. By a hypothesis we collect our knowledge about a set of phenomena into a brief statement, or, as Whewell put it, we "colligate the facts," and thereby present a multitude of experiences in a brief compass. In this respect, even a hypothesis that includes a large amount of error, such as the Ptolemaic astronomy, may be of considerable service, for deduction from it serves as a means of suggesting what observations should be made, and, in the process of checking the consequences of such a hypothesis, observation will often suggest a better hypothesis. The logical procedure of verification or disproof is intimately bound up with the procedure of discovery, and the character of the observations that bring about the disproof of one hypothesis often suggests the sort of modification that ought to be made to create a better hypothesis. An unverified hypothesis that is used chiefly for the purpose of making further discoveries, or for correcting its own errors, is often called a working hypothesis.

It is important to realize that observations are rarely made at random, and, when so made, rarely give significant results; observation is almost always guided by some suggestion or at least by a tentatively held notion. If mere observation of facts were the ideal of science, the scientist might well sit down by the side of a path and start observing the grains of sand along the walk—he could spend several years in counting the grains of sand in a handful, in carefully recording the geometrical characteristics of each grain, in making a statement of the arrangement, structure, coloring, etc., of each face of each grain—indeed, a lifetime would be too short for such a pursuit of the observable peculiarities to be found in a heap of sand. The very triviality and uselessness of such a pursuit, from the point of view of science, shows that such a frittering away of time in mere observation is not the actual procedure of any intelligent man, and that observations are always made with some objective in mind, more or less clearly defined. Endeavors to make any and every sort of observation are the exception; as a rule, only *pertinent* observations are made. It is the hypothesis that is the guide as to what observations are made and what are omitted. The hypothesis fulfils this important function in the non-logical procedure of discovery.

§ 3. While the statement of hypotheses about ordinary matters is well within the mental capacity of anyone, the more general or the more important the subject under consideration, the more difficult it becomes to conceive of an appropriate hypothesis. The most general hypotheses must wait for the genius to proclaim them, and the progress of science is held up until the genius comes forward with his new formulation. At present, the progress of knowledge concerning the nature of light seems thus to be halted by the failure of physicists to produce a hypothesis which is adequate to direct inquiry and to reconcile the contradictory observations that have been produced.[1] It is on this account that speculation is valuable, for speculation is the suggestion of possible hypotheses.

In philosophy, which deals with more general matters that are less open to verification, it sometimes seems as if the suggestion of hypotheses is alone possible. Philosophy has therefore been conceived as a sort of poetry, whose worth consists in the suggestion of possible alternative views of life—mere speculation. Nevertheless, if there is any preference between speculations, that preference should be established, as far as possible, on logical grounds; if there is no preference, why have *schools* of philosophy been established which cling to one particular view? Since the time of Kant, speculation has run riot in philosophy, and the greatness or value of a philosophy has often been measured by the criterion of whether that philosophy does or does not present *new* conceptions.

Because of the difficulty of obtaining the most general hypotheses, the man who first suggests a new idea, such as the atomic nature of matter, has often thereby conferred a great boon upon humanity. When Copernicus became dissatisfied with the complexities of the Ptolemaic system as modified to suit the increased knowledge of his day, he searched the writings of the ancients, and found in Cicero the statement that Hiketas of Syracuse said that the earth moved. This speculation of Hiketas' served to suggest to Copernicus his new hypothesis, and, because he had a more powerful means for deducing the consequences of that hypothesis than had the ancients, he was able to go further than they in verifying it and making it plausible. Thus we owe the beginnings of modern astronomy to a speculation from the ancient world.

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[1] Cf. R. A. Milliken, "Radiation and the Electron," in *Nature* 101: 234–7, 254–7, May 23–30, 1918, or F. C. S. Schiller, "Hypothesis," in C. Singer, *Studies in the History and Method of Science*, Vol. II, chap. xi, p. 437.

It should, however, be realized that speculation without verification is fruitless, and the speculator should recognize that he has a responsibility for justifying his assumptions, as well as for presenting them. Empedocles made a great contribution to human thought, but it was Dalton who first made it serviceable. Empedocles is remembered for his discovery of the element, while others of his speculations, on which he set equal store, such as his biological theories of the cause of growth in plants, have been deservedly forgotten. While imagination is an extremely valuable faculty, its exclusive use does not deserve the name of science. Philosophy is more than poetry, for it at least pretends to be dealing in truth, not merely in fairy tales.

§ 4. Although the question of the source of hypotheses is of little importance for logic, it has considerable epistemological and metaphysical interest, and may be dealt with here. If by experience we mean everything that passes through the mind, so that we include both the Lockian "ideas" and the Leibnitzian "mental structure," it can be truly said that hypotheses, which are the material for knowledge, come only from previous experience. Sometimes there is, however, great difficulty in determining from what part of experience a particular sort of notion or hypothesis comes, and this difficulty has sometimes led to strange metaphysical theories.

It is a well-known fact that most hypotheses are derived from analogy. Jevons has well illustrated this influence in the formation of hypotheses.[1] The importance of analogy in the formation of philosophic conceptions can also be traced. For example, Locke's conception of simple and complex ideas was probably suggested by his association with Bayle, and with the theory of chemical atoms and compounds; Schelling's metaphysics came from his experience as an artist; Aristotle's conception of essence, potentiality, and actuality from biological growth; indeed, careful investigation will very likely show that all philosophic theories are developed analogies. Now an analogy has this peculiarity: it fits in some circumstances and does not fit in others. Hence it is vitally necessary to distinguish in what respects each speculation or hypothesis fits and in what respects it does not fit. Otherwise those elements of an analogy which are not pertinent will mar the conception. Or we may fail to see that there are other analogies which may also be applied to the situation.

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[1] *Principles of Science*, p. 630.

When there is no closely analogical experience to be applied to the case, more distant analogies may be resorted to. Empedocles wanted to express the concepts of attraction and repulsion, but, since those terms had not yet been invented, he used terms of common experience, love and hatred, which are the analogical experiences from which the concepts of attraction and repulsion were later abstracted. This abstraction was made because it was seen that the psychological elements in that analogy were not justified in a cosmological scheme.

One of Aristotle's difficulties gives us an excellent opportunity for illustrating the method of securing a hypothesis. "If we were above the moon, and saw the earth opposite, we should not know the cause of an eclipse (of the moon). For we should perceive that it is eclipsed, but in short should not perceive *why*, since there would not be a sensible perception of the universal."[1] Aristotle correctly observed that even if we could take a station from which it would be possible to observe an eclipse of the moon, and see its disk darkened, we should not even then be able to *perceive* the reason for that darkening. In other words, the mind imports into the situation the hypothesis for the explanation of an eclipse. Whence does it come? From experience of shadows. But experience of shadows shows us only that when we come between a light and a wall, there is a shadow upon the wall. We do not even herein have any experience of the *why* of a shadow. That *why* is also a hypothesis: we assume that something called light moves in straight lines, like balls thrown or rolled (we have seen balls move in approximately straight lines), and that the light which hits our body is stopped. This idea that light is stopped is again derived from some other experience, as when a rolling ball is stopped by a stone. Thus bit by bit we piece together an explanation of our shadow and then apply it to the moon—that the shadow on the moon is analogous to our shadow on the wall. No wonder that Aristotle, seeing that even the *why* of our own shadow is never experienced, should have averred that universals never come from sense experience. The connection is sometimes very recondite and complex (we know that some primitive tribes today have not yet attained the correct explanation of a shadow), because the simplest hypothesis of daily experience draws upon a multitude of forgotten experiences. Consequently, philosophers have rarely tried to hold an absolute empiricism. The imagination, which abstracts part of one experience and imports it into another experience, is the most marvelous of human functions.

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[1] *Posterior Analytics* I. xxxi. 1.

§ 5. Most of the ideas that we use in explanation of the world can be traced to experiences or abstractions from experiences without much difficulty. But there is one class of such ideas whose origin presents especial difficulty. A great deal of metaphysical and epistemological capital has been made out of the mystery found in the origin of mathemetical, especially geometrical, concepts, particularly, in modern times, by the neo-Kantians. Such ideas as the point or the straight line, in fact, all geometrical conceptions, seem clearly not to be abstractions from experience, since a point, which has position but not thickness, or a line, which is perfectly straight, is not found in experience. Not being found in experience, it plainly could not be abstracted from experience. On the other hand, our concrete points and lines seem to be striving to be mathematical points and lines, though only approximately reaching that goal; consequently the mathematical concept seems to be prior to the straight line or regular figure. Hence it is concluded that the mathematical concept is a priori, latent in the mind, and only approximated in nature. This certainly seems to be the citadel of apriorism; if we can discover a source in experience from which these mysterious concepts come, we ought to have no difficulty in tracing all other concepts to some concrete experience.

The source of these geometrical concepts, when it is once pointed out, is very simple. They are hypostatizations of purposes. I cannot draw a straight line, but I can have the purpose of drawing a line in only one direction—most naturally in a single direction out from my eye, just as a ray of light proceeds straight to my eye. I can assume that the purpose is fulfilled and withdraw my attention from all other irrelevant circumstances, such as the breadth of the line, slight inadvertent crookednesses, etc., and then I have the concept of a mathematical line. Or I can have the purpose of putting something (for example, my finger) down upon a single place, at the same time abstracting from the covering of any spot by that finger—if I neglect the actual carrying out of such a purpose with its inevitable covering of some surface, and consider only the intention, then I get the concept of position without magnitude. These mathematical concepts have the simplicity of purposes —no physical object has the simplicity of a point or a straight line; but purposes have exactly this extreme simplicity, which is so mystifying if we assume that it comes from the world of nature. Other geometrical concepts, such as the triangle, the surface, etc., arose by their combination. The fundamental concepts of geometry and physics are thus formed from the hypostatization of simple purposes.

It is noteworthy that these concepts did not originate early, or even with the beginning of deductive geometry, but only as the result of Zeno's criticism of a Pythagorean conception which had made the point analogous to a very small pebble—possessing magnitude, though only a slight amount of it. Our present geometrical concepts are the result of a very sophisticated search for the appropriate hypothesis.

Once a concept is formed, it can be applied wherever it is found to fit: the concept of a geometrical line, which possesses only one dimension, may be applied to time; or the point may be thought of as moving along one direction to generate a line, and that to generate a surface, and that again to produce a solid, etc. Infinite divisibility is another such mathematical concept that is also the hypostatization of a purpose—the purpose of cutting a thing in two repeated over and over; in practice, we should very soon get a piece so small that it could not be cut in two; but the purpose of cutting it and the getting of two pieces, which came from previous experience, may still be applied to it, although the purpose is only carried out in imagination; thus the concept of infinitely continued division is formed.

In the search for the origin of concepts, human purposes seem to have been neglected, so that this sort of concepts has been left unaccounted for. With this explanation, we see that there is no need for turning to any a priori faculties for the origin of ideas, and that, while the universal may not be *seen* in the experience, it may easily be *imported* from one experience into another.

§ 6. Since all concepts, and hence all hypotheses, come from experience only, and since, according to our view, knowledge is attained by the proof of what were originally hypotheses, one of the limits of knowledge is determined by how far we can go in framing hypotheses. Men have been able to verify hypotheses, such as that of the atomic constitution of matter, which, to an earlier age, that of the Greeks, seemed quite unverifiable; so we dare not assume any limits to the possible verification of hypotheses. But there are limits to the formation of hypotheses, since the material for such formation can only come from experience, and human experience is limited. The range of hypotheses is dependent upon our experience both as individuals (including vicarious experience attained through social communication) and as a race.

If there is a phenomenon whose explanation requires a hypothesis which presupposes experience of a *kind* that goes *entirely* beyond the limits of human experience, such a phenomenon can never be explained. Knowledge can go only as far as imagination; if there is a sphere of nature in which the phenomena or causes are of an *entirely different sort* from those to be found in human experience, it can never be known. Perhaps that is the reason why the philosophical problem of a future life is seemingly insolvable—life without any of the phenomena which accompany our present material existence is quite inconceivable; and yet, if there is a future life, it seems to be necessarily an entirely non-material existence; hence no concrete hypothesis concerning it can be framed, and it is unknowable.

An example of knowledge that is just on the border line between the knowable and the inconceivable or unknowable is action at a distance. Perhaps one reason why modern physics developed so slowly was that it demands the constant and ever present use of the conception of action at a distance. Indeed, modern conceptions of the electrical nature of matter seem to place action at a distance as the fundamental and only real form of action! Yet action at a distance is almost inconceivable. In human life we have no experience of action at a distance—only of action by contact, as the pushing or pulling of objects. The attraction of love is so plainly a psychological phenomenon that, once out of the animistic stage, man hesitated to apply it to inanimate bodies. Even the seeming action of a magnet at a distance is explained as due to rays of force, which go out of the magnet in definite paths and so influence objects by contact. Fortunately human experience is not entirely without analogies on which action at a distance can be conceived, so that it is possible to frame the hypothesis that the earth is a magnet drawing stones to itself.

As a last resort in the formation of a hypothesis, abstract concepts can be put together without giving them any concrete filling: for example, the concept of "action" and "at a distance" may be united. Such a highly abstract concept has the difficulty of not permitting anything to be inferred from it for the purpose of deduction; it lacks content. But when a concept is once framed, it can be arbitrarily given a content, which Newton did by a mathematical formula, and in this way the difficulty can be surmounted. By such expedients men have succeeded in pushing out the borders of experience a little, and have enabled the limits of knowledge to be widened.

§ 7. This discussion of the source of hypotheses shows us that we ought hardly to expect any important new concepts to be developed from the field of pure philosophy. Men of the greatest ability have been thinking about philosophic problems for over two thousand years, but most of the important ideas in philosophy were suggested in the ancient world. The number of important philosophic ideas seems to be limited. The logical possibilities of pure thought seem to have already been pretty thoroughly explored. All that we can hope for is that some of the theories now in vogue may be verified, and that such verification will direct men's minds to thoughts which have previously been suggested, but have been comparatively neglected. Philosophy deals with nature at second hand—it is reflection upon concepts discovered by other human disciplines; natural and social science, in delving into the minute and the vast features of the world, the phenomena of life and mind, may be expected to increase human experience, and thus increase the range of concepts at the command of seekers after knowledge. Even here we can, however, hardly expect any important *new* ideas, merely new arrangements of old ideas, for we are limited to human experience for the ultimate constituents of all possible knowledge.[1]

§ 8. This discussion of the origin of hypotheses has carried us beyond the field of logic. To logic, it makes no difference whence or how a hypothesis is derived; that is a psychological process with which logic has no concern. The concern of logic is not to obtain hypotheses—that is the function of the discoverer, the philosopher or the scientist; the logician takes the hypothesis only when it has been formed, and criticizes its proof. The first hypothesis that the discoverer forms is often only very tentative; it may be only a vague guess, which he gradually modifies to fit more and more closely to the situation in the process of attempting to prove it, since he therein sees its inadequacy. Galileo started with the hypothesis that Nature works in the simplest manner. From this assumption, he deduced that bodies will fall in the simplest way, which means the addition of equal velocities in equal times. This (inaccurate) deduction again became a hypothesis, which he could put to the test; by experiment he came to state in mathematical form the amount of velocity that is added in each second, and so he obtained his law of falling bodies. For the investigator, a hypothesis may be only tentative, to be revised or modified as the result of further experiment—a working hypothesis; but for the logician, a hypothesis is merely a candidate for knowledge, and the logician must treat each of the investigator's successive statements of a hypothesis as a separate hypothesis when he tests their correctness. There is, of course, no absolute opposition between these two attitudes, for the same person is usually both investigator and logician; nevertheless, to be clear as to what we are doing, we must keep these two attitudes distinct. While the investigator is concerned with the correction of his hypothesis, the logician is merely concerned with its correctness, its truth or falsity.

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[1] Throughout the foregoing sections, our argument has been based on the assumption that knowledge comes only through the use of hypotheses, i.e., through the method of rational induction. This assumption will be justified in the succeeding chapters.

§9. Since we are chiefly concerned here with the logical problem, we may neglect other more psychological problems, whose interest may tempt us. Should it become an established fact that no hypothesis and no thought goes on without there having been a problem to direct and guide thinking, that would be a very important discovery, but it would have little or no bearing upon logic, for logic does not deal with the discovery of hypotheses or with processes of thought, but only with the criticism of those results of thought which have already been attained. We are then chiefly concerned, in this chapter, with showing that the hypothesis is used as widely as we have said it is—in the determination of all knowledge.

That physical science uses the method of hypothesis widely is admitted; such cases as the use of Mill's canons, where another method seems to be used, will be considered in another chapter, and we shall see that, without a hypothesis, Mill's canons are rarely or never used.[1] A hypothesis may be merely a suggestion that two factors may be related in some way. For example, a scientist is looking for the cause of fogs, and starts by investigating the electrical conditions of the air—here his hypothesis is merely the vague thought that the electrical condition of the air is somehow connected with the presence of a fog. When we consider the implications of such an instance, we see that hypotheses are present everywhere, and that there is little or no investigation that is not guided by a hypothesis. That type of procedure which is not guided by at least a tentative and vague hypothesis would be of the same sort as the hypothetical scientist cataloguing the idiosyncrasies of individual grains of sand without even trying to classify them—any attempt at classification could proceed only by the introduction of some type of order that is not literally found in the order of presentation of the grains of sand or other material. Thus we can see the omnipresence of hypotheses in natural science.

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[1] Chap. viii, § 4.

In philosophy, ever since the time of Kant, hypotheses have been continually used, though with infrequent recognition of their logical nature. In a preceding chapter[1] we noted the hypothetical character of German absolute idealism—Fichte conceived the universe on the analogy of a moral act, Schelling used the analogy of an aesthetic act, and Hegel conceived it as the dialectical process of thought. The rise and fall of Hegel's philosophy is one of the clearest examples, in the history of philosophy, of the application of the inductive method to philosophic proof.

Even Mr. Russell, ordinarily a devotee of the purely deductive method, changes to the inductive method when he tries to justify his belief in the existence of a realistic world—he constructs a "model hypothesis," and proceeds to show that the apparent world can be deduced from that hypothesis;[2] then he pares that hypothesis down to the minimum. Bradley's proof that the land around which he sailed was an island[3] is also inductive in nature—he proves it by making a diagrammatic

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[1] Chap. iii, § 20 ff.

[2] B. Russell, *Scientific Method in Philosophy*, especially chap. iii, pp. 87 ff.

[3] *Logic*, p. 367.

"construction" of his course, then by analogy between the spatial diagram on paper and his spatial movements in the boat, he concludes that it was an island around which he sailed. The correctness of that analogy would depend upon the correctness of the methods of cartography, which again would depend upon the experience of mariners and others who used maps, whose experience shows that a map is able to predict and cohere with their experience. If Bradley is *assuming* the correctness of the analogy between his course and his paper drawing of it, his procedure is, however, deductive, with the analogy between the map and the sailing as the major premise; if, on the other hand, he is justifying his analogy by showing that it would enable him to deduce other properties of islands, etc., his procedure is inductive—he does not seem to be clear as to just what he is doing, although the interpretation that he is justifying his analogy fits the larger part of the passage. Bosanquet has given an excellent example of the use of induction by hypothesis in a field which does not allow the use of mathematical deduction, that of comparative religion, explaining the meaning of the association of the god Apollo with the mouse.[1] In that passage he expressly states that the use of hypotheses may lead to *certainty*, in opposition to the usual view that induction leads only to probability. But he does not see that such an admission will give him a different logical theory from his own and a new theory of the nature of knowledge.

Logic concerns itself, then, only with the *use* of hypotheses, not with their discovery, and with only one particular use of hypotheses, that of establishing their truth or falsity. We must now proceed to the next step in the logical process of rational induction—the elaboration of the consequences of a hypothesis, or deduction.

[1] B. Bosanquet, *Logic*, II, 162–67.

CHAPTER VI

DEDUCTION

§ 1. Deduction is that type of reasoning in which, from the postulated or verified knowledge of a set of propositions, there is derived with certainty the postulated or verified knowledge of one or more further propositions. It is the systematized method of elaborating the consequences of a hypothesis, an essential and extremely important part of the method of rational induction. The theory of deduction, especially the Aristotelian syllogisms, has been the stronghold of the traditional logic. Consequently it would seem that deduction should not need very much discussion; but the conception of deduction as the elaboration of the consequences of a hypothesis leads to so many new insights into its nature that it will be necessary to make a detailed investigation, especially into the nature of deductive rigor and of deductive necessity.

§ 2. The term "deduction" like the term "induction" covers two distinct processes: a psychological process of thought and elaboration of what is implied in the original assumptions from which deduction proceeds and a logical process of criticizing the validity of the results that are thereby attained. We shall endeavor to confine ourselves to the strictly logical viewpoint, although it will be necessary to touch also upon the nonlogical aspects of deduction.

The psychological processes of thought by which the implications of an assumption are discovered are very obscure. Sometimes they follow approximately the procedure of the Aristotelian syllogism, as in the famous case of the abbé who was telling that his first penitent was a murderer when a nobleman came into the room and informed the company that he was the abbé's first penitent. It is consequently quite correct to call the Aristotelian syllogism a mode of thought or inference. Sometimes the process of thought in elaborating the consequences of a statement follows the procedure of mathematical deduction, as in the solution of those kinds of mathematical problems with which we are familiar. But the formal demonstration of a geometrical proposition is not the natural way of arriving at the theorem; anyone who has ever worked out an original problem recognizes that geometrical demonstration is a highly formal and artificial mode of thought. Nearly always, in dealing with unfamiliar problems, the procedure of thought is to jump to the conclusion by a flash of intuition, and then painfully to construct the steps of deductive proof. Poincaré has shown us that his discoveries were made by a flash of intuition, not step by step by deduction.

Such intuitions can never be amenable to logical rules; they are essentially lawless, flashes of genius, of immeasurable value to science, but a type of procedure that cannot be reduced to rule. Intuition cannot become a science. Science can only give advice as to the best method or methods of guiding thought; such service is of great value to the second-rate thinker, but the genius will always be independent of such advice and discover for himself his own method of thinking as well as his conclusions. So we must conclude that, in deduction as in induction, logic has no jurisdiction over modes of thought as such. The cataloguing and discussion of modes of thought must be left to psychology; logic deals only with the criticism of the results that thought has obtained. Most criticisms of the syllogism are due to a failure to appreciate this fact. Logic does not concern itself with the process of inferential *thought*, but only with the criticism of the results of such inference.

One consequence of this distinction is that for logic the distinction between the judgment and the proposition is of no importance. Since criticism does not concern itself with the process of knowing, but only with the correctness of the results of that process, it matters not whether a decision has just been reached and is a judgment, or whether it has been reached for some time and is only a proposition. That distinction is important for a psychological discussion of processes of thinking, but not for logical processes of criticism.

§ 3. The conception of deduction as the process of elaborating the consequences of a hypothesis shows us that deduction must be broader than the Aristotelian syllogisms. Hypotheses are not always framed solely in terms of the relations of inclusion and exclusion, which are the staple of the traditional Aristotelian syllogisms, nor is that always possible. Mathematical and other relations are even more common. Hence mathematical and logistic deduction is equally as important as Aristotelian deduction and often more important than it. The Aristotelian syllogism must be considered as merely one variety of deduction, along with mathematics, logistic, the *a fortiori* argument, forms of relational inference in which terms are connected by some relation other than that of inherence or class inclusion or exclusion, such as "is east of," "is older than," and other types.

Logic seeks out those forms of reasoning which are commonly used, systematizes them, and determines the conditions under which they give correct results. It can be partial only to those which are most commonly used. We shall consequently need to search for the most general conditions of the validity of deductive inference. While we shall have much to learn from the Aristotelian syllogisms, because, in that form, deduction has been worked out in greatest detail, yet it will be necessary for us to generalize our results to cover all processes of elaborating the consequences of a hypothesis.

§ 4. A consideration of the place of deduction in the inductive process shows that its extreme importance in that procedure, and also the reason so many logicians have conceived of proof as exclusively deductive in its nature. If a fundamental truth can (in most cases) be established only when it is shown that its deduced consequences are found in experience, the deduction of the consequences of a hypothesis becomes a matter of cardinal importance. With unimportant exceptions, we may say that, unless there is a deduction, nothing can be established; those hypotheses from which nothing can be deduced can never become knowledge. Furthermore, deduction is that part of the inductive process which is most easily recorded in definite form, and can be most easily checked; it also usually needs careful checking—so it is not surprising that some thinkers, like Bertrand Russell, should have conceived of the establishment of hypotheses as wholly deductive in nature.[1] The difficulty in the establishment of some hypotheses, such as the existence of God, consists in the fact that it is so difficult to deduce anything unique from such a hypothesis—we have no knowledge of how God *must* act, and so it is very difficult to say, if God exists, that consequently such and such unique events must be found in the world. Hence it will be impossible to deduce from the concept of God any happening which may not also be deduced from another hypothesis; we cannot be sure that our hypothesis is not *ad hoc*, and that it will not need to be changed again and again in the light of fresh experience.

The importance of deduction in the establishment of knowledge is also shown in the fact that it was largely because of the lack of adequate and exact methods of deduction that the ancients were unable to proceed further in attacking the problems of physics. Aristotle knew the properties of the lever; he decomposed the motion of the ends of a lever into two components, one of which moves in the direction of the tangent to the circle in which the lever moves, which he said was "according to nature"; and the other, in the direction of the center of the circle, which he said was "contrary to nature." His hypothesis was that, since there is more motion contrary to nature in the smaller circle, the mover or weight on the larger arm, where there is less motion contrary to nature, would go farther than that on the smaller arm, where there is more motion contrary to nature.[1] This is plainly a hypothesis *ad hoc*, for if we analyze it, we see that more motion "contrary to nature" merely means that the longer arm will move farther than the shorter arm—which we already know. Aristotle's difficulty was that he used the terms "contrary to nature" and "in accordance with nature," which were so vague and ill-defined, that he could not deduce anything else from the hypothesis than this one example—the vagueness of his fundamental ideas prevented adequate deduction, and so failed to lead him to see the inadequacy of his hypothesis.

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[1] *Principles of Mathematics*, p. 441.

Similarly, Aristotle explained the circular shape of the bright spot when the sun's rays fall upon a wall in a dark room as being due to their being emitted in a conical form; the base of a cone is always a circle, so that wherever the sun's rays fall, they appear as circular.[2] Had Aristotle made an exact deduction of the shape that the shadow of the sun's rays should assume, he would have seen that the interception of a pencil of rays from the vertex of a cone by an opaque object containing an irregular hole would not give a circular image, but an irregular image—his deduction from his hypothesis was incorrect, and so he did not perceive that his hypothesis could not explain the shape of the image of the sun.

In present-day science, we have another very illuminating example of the importance of deduction to science—Bohr has propounded a theory of the structure of the atom which has accounted for the behavior of atoms that are composed of two bodies—of hydrogen and ionized helium; but his theory cannot be verified for other atoms because of the inability of mathematicians to solve the three-body problem in a general form, so that where more than two bodies create a gravitational or electrical field, it is impossible to deduce what phenomena are to be expected, except in special cases. Here we have a hypothesis that seems adequate to account for the phenomena, and we have elaborate observations of phenomena, but the inability of the deductive process to deduce any phenomena from the hypothesis is preventing physics from solving this most important problem. What is needed is, evidently, not a physicist to give new theories or new facts, but a mathematical Newton to provide a new method of deduction. Deduction may almost be said to be the most important part of induction.

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[1] *Mechanics* iv.

[2] *Problems* xv. 6.

§ 5. For the past three hundred years mathematics has been developed by geniuses who were anxious to make new discoveries, to demonstrate new propositions; it has only been recently that mathematicians have seriously turned upon their science and have sought to discover the foundations upon which it is built.[1] They have talked about rigorous demonstration, and have brought forward supposed examples, only later to discover that these supposed rigorous demonstrations, while correct enough, nevertheless omitted essential steps or failed to state essential presuppositions of the proof. In the effort to advance the boundaries of mathematical knowledge, the nature of rigorous demonstration has been left vague and unsettled, to the great confusion of the science. Furthermore, mathematicians have endeavored to state what are *all* the postulates of given mathematical systems, but as yet no general method of discovering such postulates has been discovered. Russell and

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[1] §§ 5–18 are reprinted by permission from the *Monist*, Vol. XL, pp. 94 ff.

Whitehead have given us a list of what they declare to be all the primitive propositions upon which the extensive system in *Principia Mathematica* rests; but they, too, have failed to find any general method of discovering how many or what are the presuppositions of an argument. If we could find out what are the requirements of completely rigorous deductive proof, we should have such a method. We must now face that problem.

Let us provisionally define a rigorous demonstration as one in which all the presuppositions of the proposition to be demonstrated are stated clearly, and in which these presuppositions are obviously combined to produce the required result. Then let us see to what concrete formulation such a criterion will lead us. Certainly there could be no stricter requirement of rigorous reasoning than the one we have given, and we are seeking for the characteristics of the most rigorous and exact deductive proof.

In the modern world, mathematicians have not dealt explicitly with this problem; but in the ancient world, Aristotle wrestled with it. He was troubled by the disputations of his contemporaries, and endeavored to discover a scientific method of reasoning that would determine just what inferences could be correctly drawn from given premises. He thus discovered the syllogism—the first attempt to state the requirements of rigorous proof. But Aristotle, while a competent mathematician, was not interested in the philosophy of mathematics. His logic was formulated as the result of an attempt to give to the dialectical and rhetorical disputations of his day a scientific methodology—it had nothing to do with mathematics. Its purpose was ultimately practical, the settling of disputes, rather than theoretical, the statement of the general conditions of proof. Thus formal logic and mathematics were separate in their origin. They have since pursued opposite courses. In the modern period, Descartes, whose influence upon mathematics was as profound as upon philosophy, accentuated that separation. He despised the Aristotelian logic of the schools as being entirely useless, and tried to originate an intuitive logic of his own, modeled after his own procedure in his mathematical discoveries. Thus mathematics and logic have been separated. Yet both pursue the deductive method, and those mathematicians who have attempted to state the fundamental postulates of any branch of mathematics have been obliged to confess that these postulates are combined in accordance with the laws of formal logic. In such a situation, is it any wonder that the nature of that logical rigor upon which mathematics prides itself should have remained entirely vague? Let us try to enter into the Aristotelian logic in the spirit of the mathematician, and then apply what we find there to mathematical reasoning. By this method we may come to an appreciation of the nature of rigorous demonstration.

§ 6. First, we must test the rigor of the Aristotelian syllogism. Suppose we ask ourselves the question, How many propositions are implied in the drawing of a syllogistic conclusion? In Aristotelian inference, we have been accustomed to say that in a syllogism two propositions are necessary as premises, and that in immediate inference only one is needed; but that if more than two are needed, the argument can be put into the form of a set of syllogisms or immediate inferences. In addition we have talked about the *dictum de omni et nullo*, or of the *nota notae;* but what relation have these propositions to the premises of the syllogism? Certainly they are not premises, yet they seem to be involved in the syllogism. Similarly, in mathematical inference we have reasoned that if $x=y$ and $y=z$, therefore $x=z$, but we also have the statement, "Things equal to the same thing are equal to each other," which does seem to be involved in the reasoning, yet is plainly not one of the premises.

A convenient and easy means of discovering the presuppositions of any argument is to ask ourselves the question, What propositions can be found which are both deductively independent of all other propositions that are presupposed by the piece of inference we are analyzing and the separate denial of which renders the conclusion false or invalid? If the denial of any proposition changes the truth-quality of the conclusion, that proposition is plainly presupposed by the conclusion; either mediately, through some other proposition, or directly. If then we eliminate all propositions from which we can deduce those that we have already found to be the direct presuppositions of the conclusion, we shall have left the few which are directly involved in our argument.

§ 7. By applying this criterion to an ordinary Aristotelian syllogism, as, for example, "All ruminants part the hoof," "All deers are ruminants," therefore "All deers part the hoof," we find that both premises are necessary to draw the conclusion, for, if either is denied (or any proposition from which either could be deduced), the conclusion would not necessarily follow. Similarly, there is a third proposition or *canon* of combination of the premises, which, if denied, would independently prevent us from asserting the positive truth-value of the conclusion. This proposition is the rule for combining the premises of a syllogism in *barbara;* for, if we make the experiment of denying the rule for combining the premises in *barbara*, that is, if we deny that *barbara* gives a valid syllogism, the conclusion does not follow. Yet the rule for drawing the conclusion of a syllogism in *barbara* (or any more general rule of which it is a particular case, and which would be denied in denying it) is the only rule of combination that would apply to this syllogism; for this syllogism presents the characteristics of a syllogism in *barbara* and of no other. Any other statement that we attempted to put into the place of the rule for the syllogism in *barbara* would be equivalent to that canon, and would be denied in denying it; if we used any other non-equivalent canon, the premises would not fit such a canon. Were we to change the form of one of the premises and use the rule of the hypothetical syllogism, we should no longer have this syllogism, but another, because we should have a different set of premises, though they might mean the same. Thus the canon of combination of premises is one of the presuppositions of the conclusion of a syllogism.

Just how the canon of the Aristotelian categorical syllogism should be stated has been discussed by logicians, but its function has not been understood, and consequently no definite agreement has been reached. The *dictum de omni et nullo, Quod de aliquo praedicatur, praedicatur etiam de qualibet eius parte*[1] is the medieval formulation of Aristotle's statement of the canon of syllogistic reasoning. Since Aristotle regarded all forms of syllogisms as reducible to the first figure, he stated the canon in a form applicable to that figure only, "What is predicated (stated or denied) about any whole is predicated (stated or denied) about any part of the whole." As this canon seems to necessitate the syllogism being understood in extension, the *nota notae* was formulated for the syllogism when understood in intension, *Nota notae est nota rei ipsium* and *Repugnans notae repugnat rei ipsi.* Mr. Joseph has derived a formulation from Kant's statement that the syllogism subsumes a cognition under the condition of a rule, "Whatever satisfies the condition of a rule falls under the rule,"[2] but this formulation may be objected to; it is usable only in syllogisms with an affirmative minor premise; the terms "condition" (which seems to refer to the middle term) and "rule" (which seems to refer to the major premise) are unnecessarily abstract and used in a confusing manner.

The relations between the terms of a syllogism that is understood in extension are different from the relations of a similar syllogism when it is understood in intension. This difference is apparent when the syllogism is stated in exact symbolic terms. That fact shows us that the Aristotelian categorical syllogism is essentially ambiguous, and that the same formula can be used for different types of reasoning—this characteristic of the Aristotelian categorical syllogism is probably the reason why it has been so difficult to formulate its canon. When the major premise of an Aristotelian syllogism is a definition or a universal affirmative proposition which is convertible *per simplicem*, we have still another variety of syllogism, for it can now be used to draw conclusions which are otherwise impossible; it now needs a different canon from that used ordinarily. Either we must give the categorical syllogism a set of canons for the different types of reasoning expressed in it or we can state the canon in the clear but cumbersome and ambiguous form, "In an Aristotelian categorical syllogism, when the eight syllogistic rules are all observed, the major term may be predicated (asserted or denied, as the rules require) of the minor term."

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[1] Cf. H. W. B. Joseph, *Introduction to Logic*, p. 296, note.

[2] *Ibid.*, p. 309.

§ 8. Our list of presupposed propositions does not stop with the premises and canon. By applying our criterion for finding the presuppositions of an argument, we find a fourth proposition is also needed for the complete necessity of the reasoning, namely, the proposition, "The canon of the syllogism in *barbara* is correctly applied in this syllogism and the conclusion is derived in accordance with it." This may be called the *validating proposition*, for its correct assertion makes certain that the canon has been validly applied to this case of reasoning. This validating proposition or its equivalent is presupposed in the syllogism we have been analyzing, for its denial renders the syllogism of no cogency, and, on the other hand, it is not logically implied by any other of the four propositions in the syllogism or by any other proposition presupposed by any one of those four. Without it (and its proof) or its equivalent, we could not be sure that our reasoning is valid, that is, that the premises are of the sort to which the canon is applied, that the canon is correctly applied to the premises, and that the conclusion is correctly derived by that process. Any other proposition or set of propositions from which we endeavor to derive the correctness of the syllogistic reasoning would be equivalent to this statement or part of what is included in it, and would be denied in denying the validating proposition. In asserting that the truth of the conclusion follows from the truth of the premises, we are not merely asserting that the relationship expressed in the conclusion exists, but that we *know* that it must necessarily exist, that is, that our deduction is *correct*. The validating proposition is the making explicit of this implied notion, which is found in every syllogism; hence the validating proposition is a necessary part of every syllogism.

For many purposes, however, the validating proposition may be neglected, because the mere statement of a validating proposition adds no *content* to the reasoning, any more than a signed statement that an automobile for sale has been examined and found in good mechanical condition adds anything to the perfection of the machine; yet, if such a statement is made by a trustworthy person, it rightly adds much to our confidence in the machine and our willingness to pay a good price for it. So it is not the statement of the validating proposition which is important, but its *proof*, since without that some fallacy may be lurking in the argument. It is for the purpose of checking the truth of the validating propositions that a critic usually goes over a series of apparently valid demonstrations. In a formally invalid syllogism, it is the validating proposition that is wrongly asserted.

The proof of a validating proposition is not to be attained by deductive reasoning. If we should attempt to establish the truth of a validating proposition by syllogistic or any other type of deductive inference, we should find ourselves involved in an infinite regress. No amount of merely syllogistic inference could establish such a proposition as "This syllogism has a categorical major premise with two terms and a copula," which is plainly involved in the proof of the validating proposition of a syllogism in *barbara*. If such a proposition were the conclusion of another syllogism, we could only base it upon some such reasoning as "The presence of certain marks indicates a categorical major premise" and "This syllogism has those marks." Then the proof of this minor premise would need other premises, such as "Some other marks indicate the presence of the first set of marks" and "This syllogism has those marks." The major premises to those prosyllogisms might be included in a set of primitive propositions, but the minor premises would give difficulty; we could continue finding marks to distinguish marks indefinitely without establishing the final truth of our minor premise. The only deductive proof of a proposition is by finding that it is implied by other propositions; syllogism deals only with propositions. No train of purely syllogistic or deductive inference can attain to the knowledge of a singular proposition unless there is a singular proposition asserted in the beginning. But it would be extremely unsuitable to include in a set of primitive propositions a singular proposition for each syllogism in the system! Exactly similar difficulties would be met with in the attempt to establish, by deduction, any other singular proposition, such as "This is a chair." Thus the deductive proof of a validating proposition is impossible, because the attempt to establish its truth leads to an infinite regress of singular propositions.

This infinite regress does not mean that the proof of a validating proposition is impossible, or that the validating proposition does not belong in a syllogism, but merely that the proof of the validating proposition is by an inductive and not by a deductive method; for certainly we do establish the truth of validating propositions when we go over a set of syllogisms and see for ourselves that the premises do correctly imply the conclusions, or when we check up the reasoning in a logistic demonstration and see that the processes of substitution or other inference are carried out correctly. How, then, is the truth of that implied validating proposition seen? Plainly by a process of mental comparison, which is essentially the same as what we shall call "immediate description." We hold in mind the two premises of the syllogism together with the canon of their combination and the conclusion, and observe that in this case the work of combination is performed correctly. There is no mystery about such a comparison. Every careful reasoner who uses the syllogism has often performed it. It is possible because all the entities concerned are mental, and because a small number of propositions can be held in mind simultaneously, so that their agreement can be perceived by a direct act of comparison. The material offered for synthesis is compared mentally with the definitions and rules which state the requirements for that type of syllogism, and direct observation is made whether the material fits the requirements or not. For example, in the proof of the validating proposition in our example, it would be directly observed that the major premise is a categorical proposition and that it contains two terms united by a copula, that the minor premise is also a categorical proposition, that one of its terms is identical with one of those in the major premise, and that the conclusion is also a categorical proposition which repeats the two terms of the premises and unites them in the proper order by a copula. It would also be observed that the syllogism has only three terms, that the middle term is distributed at least once, that there are no negative or particular premises, etc. Thus we see that the definition of the formal processes of reasoning and of the kinds of propositions, terms, etc., that are required in any particular type of syllogism, is an essential part of the presuppositions of a syllogistic argument. The definitions and the rules of what has been called "formal logic" are needed in testing the validity of an argument that conforms to any of the traditional types. It must be recognized, however, that such a direct act of comparison as is involved in that testing, is not itself a deductive method of proof, since it does not derive the truth of its conclusion from the truth of one or more further propositions, but from the direct act of comparison itself. We must then admit that every syllogism contains one proposition whose proof is by a non-deductive method.

In ordinary reasoning, we do not usually express any more of our reasoning than is necessary, usually no more than what we think likely to be challenged; hence the validating proposition has been neglected, because each syllogism is supposed to be valid merely because of its statement as a syllogism. The existence of formally invalid syllogisms should have taught us that no syllogistic argument can be complete without proof of its validity, and a statement that the process of checking its validity has been performed—the task which is performed in the correct assertion of the validating proposition. When an attempt is made to state all the propositions that are implied in a syllogism, the validating proposition is clearly seen to be an essential part of every syllogism.

It was the absence of this validating proposition which enabled Mr. F. C. S. Schiller to make the objection that no syllogism can yield absolute certainty or compel assent, because even the most faultless of syllogisms is liable to the defect of ambiguity of the middle term.[1] When the validating proposition is not even implicitly affirmed, not only is the defect of ambiguous middle possible; any other of the formal fallacies and many of the material fallacies are also possible—all of which Mr. Schiller failed to mention. Once the validating proposition is proved to be true, Mr. Schiller's objection is no longer possible, for the verification of the validating proposition eliminates any possibility of an ambiguous middle or of any other fallacy. Mr. Schiller's objection is a corollary of his thesis that induction can give nothing more than probability. Our proof that a validating proposition is a necessary part of every syllogism shows that the worth of deductive processes depends upon induction, rather than the reverse, that nothing can be established by deduction alone, that deduction contains an inductive element, and that unless certainty can be had by induction or some other non-deductive process, no certainty can be obtained by deductive means.

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[1] *Formal Logic*, chap. xvi, sec. 6.

Most logicians are convinced that a syllogism can be proved to be valid, in most cases by simple inspection, and such is shown to be the case by the successful and unquestioned use of deductive reasoning by philosophers ever since the time of Aristotle. It is, of course, true that particular individuals have made mistakes in asserting the truth of validating propositions, just as particular astronomers have made incorrect observations, but that fact should not make us any less sure of logical than of astronomical facts. Mr. Schiller's objection means that no syllogism can be proved to be valid, not even the traditional one which concludes that "Socrates is mortal"! If there were any fallacy in that syllogism, some one of the thousands of logicians who have used that syllogism since the time of Aristotle, and who have been well acquainted with the fallacies to which syllogisms are liable, including the fallacy of ambiguous middle, would have long ago pointed out any such fallacy!

§ 9. We can now state in full form the Aristotelian syllogism that we used as an example:

Major premise: All ruminants part the hoof.

Minor premise: All deers are ruminants.

Canon: In any Aristotelian categorical syllogism, when the eight syllogistic rules are all observed, the major term may be predicated (asserted or denied, as the rules require) of the minor term.

Validating proposition: This canon is correctly applied to this syllogism and the conclusion is derived in accordance with it.

Conclusion: All deers part the hoof.

The proof that all of these five propositions are needed in this or any similar syllogism[1] is that the denial of any one of the first four, even when the other three are affirmed, renders the syllogism of no probative value; and that no other proposition could take the place of any of these five propositions except a proposition which is equivalent to it and would therefore be denied in denying it. Furthermore, these four propositions are deductively independent of each other, since no one can be inferred from any of the others, and since the denial of no one leads to a denial of any other.[2] We may call the first two the *material propositions* or *premises*, since they contribute the *content* to the reasoning, and the next two the *formal propositions*, since they have to do with the *operations* performed in the syllogism. There can be no more than these five propositions in any such syllogism, since the two premises contain all the material content found in the conclusion, and any further content that might be found in the conclusion would also have to be found in the premises or else the syllogism would be invalid.[1] The two formal propositions state the entire content of the operation performed in the syllogism, and sum up all the presuppositions of that operation. If there could be any more formal propositions, it would be possible to have some new kinds of formal fallacies in the syllogism, different from any of those already known. But it may be safely concluded that if there were any such, some of the logicians since Aristotle would have discovered it.

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[1] The validating proposition and the canon of any syllogism may be combined into one proposition and thus the number of propositions in the syllogism reduced by one. However, that combination would rob the canon of its generality, and make it applicable only to this syllogism and no other. To establish such an individual validating-canon, the two propositions that we have called the canon and the validating proposition would be needed, and so the number of prosyllogisms would be merely increased.

[2] Since the validating proposition *contains* the canon as one of its parts, the denial of the canon might seem to render necessary the denial of the validating proposition; but such is not the case, since the canon is not *asserted* in the validating proposition, but merely *considered*. The removal or change of the canon would necessitate a change in the validating proposition, but its mere denial would not do so.

While our reasoning has been concerned with this particular syllogism, yet it has been in no way dependent upon this example; it has been general throughout, just as the geometrician uses a particular figure to illustrate his reasoning, but does not depend upon it for the truth or generality of his reasoning. So we may safely infer from the general character of our argument that every Aristotelian categorical syllogism has five and only five propositions. A like argument would show that the other forms of Aristotelian syllogisms, whether hypothetical, disjunctive, or in any other of the traditional forms, are similar in nature and also require formal propositions, similar to those we have discovered in the categorical syllogism, to state all that is implied in the argument in addition to the material premises. Thus our proof is complete that every Aristotelian syllogism contains five and *only five* propositions.

We have now put the Aristotelian syllogism into a form in which reasoning by means of it may be said to be completely rigorous, since we have succeeded in stating all the presuppositions of the resulting conclusion, and have put them into such a form that their combination and the manner thereof is clear and open to inspection. Deductive inference is therefore not a mysterious, indefinable operation; it can be defined and described in terms of the analysis we have made. Aristotelian inference in its logical (not psychological) nature is merely a combination of statements into another statement in accordance with an accepted rule. We shall see later that this definition holds of all deductive inference. We have further found that the cogency or apodictic necessity of a syllogism or syllogistic reasoning is not due to any mysterious form into which the reasoning is thrown, but to the implicit (or explicit) assertion of the canon and the validating proposition of each syllogism, which render the drawing of the conclusion logically inescapable and hence necessary. The "form" of the two material propositions is merely a device to enable the formal propositions to work upon the material propositions to produce the conclusion. Any syllogism which does not explicitly state all five of its presupposed propositions should be called an enthymeme.[1] An Aristotelian syllogism becomes completely rigorous when both its formal and its material propositions are stated and their truth is established.

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[1] The number of premises might be increased to three or more and all of them combined into one conclusion by a different canon. Such reasonings are found, but, for the sake of simplicity, we shall neglect them, since they can always be decomposed into a syllogism which has only two premises, and the conclusion of that syllogism combined with one of the remaining premises, etc. Complex reasonings of this sort may save time, but perhaps another name, other than "syllogism," should be given to them.

§ 10. A similar analysis of the traditional or Aristotelian immediate inference (which might well be called "Aristotelian transformation") shows that it too has its formal propositions. In such an inference as, for example, from the premise, "No men are immortal," to the conclusion, "No immortals are men," we find, by the use of our rule for discovering the presupposed propositions, that there is presupposed, in addition to the convertend, the canon of conversion, "An *E* proposition may be converted simply," and also a validating proposition, "The canon for the conversion of an *E* proposition is correctly applied to this convertend." The proof of this validating proposition depends upon a similar holding in mind of the material under consideration and careful observation that the canon is correctly applied to it and the conclusion thereby produced. We may then define immediate inference (or transformation) as reasoning that proceeds from only one premise to a conclusion, with the help of two formal propositions, a canon and a validating proposition. We may likewise define the syllogism as reasoning that proceeds from two premises to a conclusion with the help of two formal propositions. Leibnitz showed that it is possible to throw any immediate inference into the form of a syllogism, by the use of a tautologous proposition as one premise.[1] Thus immediate inference can be considered as a means by which certain commonly used sorts of syllogistic inference may be abbreviated, through a change in the canon used to derive the inference.

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[1] Consequently the number of types of categorical enthymemes is increased to sixteen; since, of the three propositions, the two material premises and the conclusion, two must always be present, certain types of enthymeme, e.g., where the minor premise and the conclusion are absent, are impossible. Otherwise the entire syllogism cannot be reconstructed from the data given.

We have thus found, by an analysis of the Aristotelian syllogism, that every syllogism contains a definite number of propositions, namely, five, which are discovered by the process of searching for all the deductively independent propositions whose denial would invalidate the inference. These five are: the two material propositions or premises; the two formal propositions, namely, the canon of inference and the validating proposition; and the conclusion. Immediate inference is found to contain one less premise, but otherwise to be analogous to the syllogism. In this way these two traditional forms of inference become modes of completely rigorous reasoning. With this analysis, let us turn to mathematical and logistic deduction, and see if we cannot find a similar condition there.

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[1] *New Essays*, Bk. IV, chap. ii, sec. 1.

§ 11. Aristotle, in defining the syllogism as "discourse in which, certain things being posited, something else than what is posited necessarily follows on their being true," was not defining the syllogism so much as stating the general characteristic of all deduction. The term "syllogism" has been reserved for that type of inference which has two premises and a conclusion, and for the varieties of that type which Aristotle enumerated. Since his time, logicians, following the lead of the mathematicians, have discovered other types of deductive reasoning, which are of greater usefulness, and the traditional syllogism has fallen into disrepute. The logician needs a word to refer to any type of deductive inference which combines two premises into a conclusion; so we may well use the term "syllogism" to refer to such a piece of reasoning, and distinguish those forms of inference that Aristotle enumerated, which have been traditionally called the "syllogism," as the "Aristotelian syllogisms," in contrast to other types of syllogisms. Such will be the meaning of these terms here. We shall find that these other types of syllogisms are essentially similar to the Aristotelian syllogisms.

A careful analysis of what has been called "non-syllogistic inference" shows that it can always be stated in the form of two premises and a conclusion; for, wherever more than two propositions are combined, two of them may first be combined, and then the remaining propositions may be combined successively with the conclusion of the preceding combination. Study of each case of combination will show that, like the Aristotelian syllogism, a canon of inference and a validating proposition are implied—the analogy between the Aristotelian syllogism and the different sorts of "non-syllogistic inference," when analyzed into sets of combinations of two premises, is so close that it is no misnomer to call them syllogisms. We may then say that the syllogism (together with its abbreviation, transformation, or immediate inference) is the unit of deductive inference. For example, let us take the relational inference that because "John is older than Mary" and "Mary is older than William," therefore "John is older than William." When we search for the propositions which are deductively independent and the separate denial of which renders the conclusion false or invalid, we find that in this inference there is implied a canon, "In inferences in which the terms of two propositions are joined by an asymmetrical transitive relation to a middle term, the subject term of the first premise may be asserted to have this relation to the predicate term of the second premise," and also a validating proposition "The above canon is correctly applied to this reasoning." In such relational inference it is difficult to call either premise major or minor; we may perhaps call the one whose predicate contains the middle term the *first premise*, and the one whose subject is the middle term the *second premise*. Such is the meaning of these terms in the statement of the foregoing canon.

Analysis of mathematical reasoning will show a similar condition. Such an analysis enables us to clear up misconceptions about the logical function of certain long-recognized propositions. For example, let us consider the inference that because $x=y$ and $y=z$, therefore $x=z$. Evidently the axiom, "Things that are equal to the same thing are equal to one another," is involved in this piece of reasoning, but just how has not been clear; it has been both asserted and denied that the axiom is the major premise. The foregoing analysis of syllogistic inference shows us that the two material premises are $x=y$ and $y=z$, and that the axiom is really the canon of inference by which the two premises are combined. A validating proposition is also implied in the argument, as the experiment of denying it will show. We can now state this piece of reasoning in full as follows:

First premise: $x = y$

Second premise: $y = z$

Canon: Things that are equal to the same thing are equal to one another.

Validating proposition: The foregoing axiom of equals is correctly applied to this syllogism.

Conclusion: $x = z$

Logistic inference shows the same characteristics. We pick at random a brief demonstration from Mr. Lewis's exposition of the Boole-Schroeder algebra of logic for analysis:

THEOREM 2.6: If $a \subset 0$, then $a = 0$*

Major premise: $a \subset b$ is equivalent to $a \cdot b = a$ 1.9

Minor premise: 0 may be taken as equivalent to b†

Canon: In any general proposition, any values of the variables in that proposition may be substituted for the variables, provided the substitution is made in each case of the occurrence of the variable.‡

Validating proposition: The canon of substitution of values is correctly applied to this syllogism.

Conclusion of this and *major premise* of the next syllogism:

$a \subset 0$ is equivalent to $a \cdot 0 = a$

Minor premise: $a \cdot 0 = 0$ 1.5

Canon: Equals may be substituted for equals.§

Validating proposition: The canon of substitution of equals is correctly applied to this syllogism.

Conclusion: $a \subset 0$ is equivalent to $a = 0$. Q.E.D.

* *A Survey of Symbolic Logic*, p. 123.

† A restatement of the usual form, Let $b = 0$.

‡ This important canon of substitution of values is used throughout by Mr. Lewis, but is only referred to on p. 357,(7).

§ Cf. *ibid.*, p. 357,(6).

We thus see that mathematical and logistic reasoning are quite analogous to the Aristotelian categorical syllogism,[1] with the difference that the relations between the terms of each proposition and the canons of inference are (usually) different. Here, again, two material propositions or premises and two formal propositions, the canon of inference and the validating proposition, are necessary to draw a conclusion.

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[1] Cf. Appendix to this chapter for a similar analysis of a logistic demonstration from *Principia Mathematica*.

§ 12. The analogues of Aristotelian immediate inference in mathematics and logistic are interesting. They are found where some rule of operation is applied directly to a proposition without the aid of a third proposition. In this type of inference, scientific mathematics and logistic have been more interested in the canons of operation than in drawing particular inferences, and so their canons have been stated explicitly. But Aristotelian logic, in harmony with its genesis and tradition of alliance with rhetoric and argumentation, has concentrated upon the individual transmutations, rather than upon the canons. As an example of non-Aristotelian immediate inference (transformation), let us take a problem from Mr. Lewis's exposition of the classic algebra of logic:

Premise: $F(x,y) = pxy + q{-x}y + 0x{-y} + 0{-x}{-y}$

Canon: The negative of any function, in the normal form, is found by replacing each of the coefficients in the function by its negative. 6.49*

Validating proposition: The foregoing function is in the normal form and the canon is validly applied to it.

Conclusion: $-F(x,y) = -pxy + -q{-x}y + x{-y} + -x{-y}$

Thus any application of a rule of operation results in a case of immediate inference. This is often the method used in the important mathematical process of transformation, and, because of the unsuitability of the term "immediate inference," it may be better to call it "transformation."

§ 13. With this understanding of the syllogism and of transformation (immediate inference), which are the two fundamental forms of all deductive reasoning, we can make an analysis of a piece of mathematical reasoning. To the mathematician such an analysis will seem unduly prolix, because the ideal of mathematical elegance is to be brief; but only in this prolix manner can all the elements involved be exhibited. It will be seen that it falls easily into the fundamental forms of syllogism and immediate inference. We have purposely selected a trite example, because it illustrates so well the principles involved.

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* *A Survey of Symbolic Logic*, p. 142.

To solve $x^2-3x+1=0$ for x, by the formula for quadratic equations:

A syllogism of substitution of values

Maj. prem.: If $ax^2+bx+c=0$, then

$$x=-\frac{b}{2a}\pm\frac{1}{2a}\sqrt{b^2-4ac}\,.$$

Min. prem.: 1 may be taken as the value of a, and -3 may be taken as the value of b, and 1 may be taken as the value of c[*].

Canon: In any general proposition, any values of the variables in that proposition may be substituted for the variables, providing the substitution is made in each case of the occurrence of the variable.

Val. prop.: The canon of substitution of values is correctly applied to this syllogism.

Concl. and maj. prem.: If $x^2-3x+1=0$, then

$$x=-\frac{-3}{2\times 1}\pm\frac{1}{2\times 1}\sqrt{(-3)^2-(4\times 1\times 1)}\,.$$

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[*] This minor premise is a restatement of the usual mathematical form, Let $a=1$, $b=-3$, $c=1$. It is composite, and, to be quite strict, it should have been divided into three propositions and the canon of inference applied three times with three different validating propositions, making three separate syllogisms. But the reasoning is quite prolix without doing that, and, since it is clear what the form of reasoning is, it has been left in the form of one syllogism with a composite minor premise.

A hypothetical syllogism

Min. prem.: $x^2-3x+1=0$

Canon: The assertion of the antecedent of a hypothetical proposition permits the valid assertion of the consequent.

Val. prop.: The canon of the hypothetical syllogism in the *modus ponens* is correctly applied to this syllogism.

Concl. and prem.:

$$x=-\frac{-3}{2\times 1}\pm\frac{1}{2\times 1}\sqrt{(-3)^2-(4\times 1\times 1)}\ .$$

Immediate inference

Canon: A number multiplied by unity gives itself.*

Val. prop.: The foregoing canon of multiplication by unity is correctly applied to this inference.

Concl. and maj. prem.:

$$x=-\frac{-3}{2}\pm\tfrac{1}{2}\sqrt{(-3)^2-4}\ .$$

A syllogism of substitution of equals

Min. prem.: $(-3)^2=9$

Canon: Equals may be substituted for equals.

Val. prop.: The canon of substitution of equals is correctly applied to this syllogism.

Concl. and maj. prem.:

$$x=-\frac{-3}{2}\pm\tfrac{1}{2}\sqrt{9-4}\ .$$

A syllogism of substitution of equals

Min. prem.: $9-4=5$

Canon: Equals may be substituted for equals.

Val. prop.: The canon of substitution of equals is correctly applied to this syllogism.

Concl. and prem.:

$$x=-\frac{-3}{2}\pm\tfrac{1}{2}\sqrt{5}\ .$$

Immediate inference

Canon: The product of like signs produces a positive sign.

Val. prop.: The canon of like signs is correctly applied to this inference.

Concl. and maj. prem.:

$$x=\tfrac{3}{2}\pm\tfrac{1}{2}\sqrt{5}\ .$$

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* To be quite strict, there should be as many applications of the canon of multiplication by unity as there are cases in the premise that a number is multiplied by unity, and thus there should be three successive and similar immediate inferences with different validating propositions for each one.

A syllogism of substitution of equals

Min. prem.: $\frac{3}{2}=1\frac{1}{2}$

Canon: Equals may be substituted for equals.

Val. prop.: The canon of substitution of equals is correctly applied to this syllogism.

Concl.:

$$x=1\tfrac{1}{2}\pm\tfrac{1}{2}\sqrt{5}\,.$$

Of course a mathematician would be extremely impatient at such prolix figuring; even a bright beginner would probably have jumped from

$$x=-\frac{b}{2a}\pm\frac{1}{2a}\sqrt{b^2-4ac}$$

and $x^2-3x+1=0$ to

$$x=1\tfrac{1}{2}\pm\tfrac{1}{2}\sqrt{5}$$

at one step. He would thus have illustrated the way the practical reasoner thinks. But he would not have been able to show the wealth of mathematical thought that underlies such a simple process as solving a quadratic equation by formula, and he would not have more than faintly conceived the number of mathematical and logical propositions that require demonstration before such a simple process can be made completely rigorous. By the time a rigorous reasoner has demonstrated all the minor premises, together with the canons (where they are not primitive propositions), in this simple piece of reasoning, he would have been carried through much of algebra and arithmetic, as well as much of formal logic. Were he to include the validating propositions in his systematic verification, he would also have had to search for the definition of each type of syllogism and propositions, and the proof could only be by a nondeductive process, which would consist in observing that the syllogism and its parts correspond to what would be expected of a valid syllogism.

§ 14. Further investigation would show that syllogisms may be classified in accordance with their canons. It is an important and an as yet largely unaccomplished task for logicians to state and classify the canons of different types of syllogisms, in order that different types of inference may be distinguished and the requirements of each type determined. As long as only one type of syllogism was allowed—that of the Aristotelian categorical syllogism in the first figure—and as long as all other forms of inference, even the hypothetical and disjunctive syllogisms, were considered to be merely forms of categorical inference, this neglect of the canon of reasoning might have been excusable. Now that other types of syllogisms have been widely recognized and regularly used by logicians, especially in symbolic logic, it is essential that they be analyzed and enumerated.

§ 15. Another investigation would also show that immediate inference (or transformation) and the syllogism are by no means independent of each other, but that the first is a means of abbreviating the second. If the major premise of the first syllogism in the last illustration had been stated in a suitable manner,[1] it could have been used as the canon for an immediate inference, and the conclusion of the second syllogism could have been drawn directly. Likewise, the two immediate inferences could have been stated as syllogisms by the use of suitable minor premises. Logistical and mathematical transformation, like Aristotelian immediate inference, is thus seen to be a method of abbreviating certain kinds of commonly performed operations, which could also be stated as syllogistic demonstrations, through a change in the canon of inference.

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[1] As, for example: In any quadratic equation which is stated in the normal form, i.e., with the coefficients of x^2, x^1, and x^0, summed, and the whole made equal to 0, x will be equal to the negative of the result of dividing the coefficient of x^1 by twice the coefficient of x^2, plus or minus "the product of 'unity divided by twice the coefficient of x^2' by the square root of 'the square of the coefficient of x^1 less "four times the product of the coefficient of x^2 by the coefficient of x^0." ' "

§ 16. It should not be supposed that a proposition which is used as a canon of deductive inference is sacrosanct and cannot be used for any other purpose. While many canons are primitive propositions in most deductive systems, yet in a well-developed system these canons may become premises as well as canons and other propositions inferred from them. An analysis of the procedure in *Principia Mathematica* will bear out this statement. This work takes the canon of the hypothetical syllogism as one of its primitive propositions,[1] and from it and some other canons (which are assumed and not stated[2]), it deduces the canons of the categorical and other syllogisms.

§ 17. If now we accept the conception of deduction given in the definition at the beginning of this chapter, we can prove generally that all deductive reasoning is syllogistic (in the broader sense of the term), that each syllogism contains the five propositions which we have enumerated, and that completely rigorous deduction consists in throwing the reasoning into the form of syllogism and immediate inference.

Any deductive inference, unless the conclusion has the same or similar meaning as the premise (i.e., unless it is an immediate inference), proceeds from one statement to another, i.e., from one proposition to a different one. But if there is a difference of meaning between the conclusion and the premise, other than a difference in statement of the same meaning, that difference can only be due to the presence of one or more additional ideas (new relations or terms). Now the introduction of a new idea can be justified only by the presence (implicit or explicit) of an additional organized meaning or meanings, which can be stated in the form of one or more propositions. Thus any inference that is more than the repetition of the same meaning in different words can only be justified by the use of at least two propositions, which contain the meanings that are combined to produce the conclusion. If more than two propositions are needed, either the set of propositions can be put into the form of a series of proofs, which produce the conclusion expressed in the piece of reasoning under consideration, or else these propositions can be combined in such a manner as to leave only two propositions (or both). Then, in any deductive inference, it is always possible to sum up the material presuppositions of an inference (providing it is not an immediate inference) into two propositions, and never possible to state these presuppositions in one proposition alone. Hence two material propositions are necessary for every deductive inference (except immediate inference), and the syllogistic character of all deduction is established.

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[1] *Principia* *1.11.

[2] These are chiefly the various canons of substitution—of values, of equals, of definiens and definiendum, etc. Cf. Appendix to this chapter.

If two propositions (material propositions or premises) are combined in some way to make a third, or conclusion, this combination must be made by some rule. Combinations, if they are to be cogent, cannot be made at random, but only in accordance with some accepted orderly rule. This rule is evidently one of the presuppositions of the inference, and, whenever we are testing the rigor of an inference by bringing to light its presuppositions, this rule or canon must be expressed. Furthermore, this rule or canon must be correctly applied to the two material propositions under consideration. The correctness of such an application will depend upon the nature of the inference under consideration and the sort of material needed for such an inference, upon the correct use of the rule in dealing with the material, and upon the correct derivation of the conclusion by that application. The correct application of the canon to the material cannot be taken for granted; the existence of formally invalid inference, where the material and the rule may be expressed, but the material does not fit the rule, warns us against merely assuming the correctness of inferences. Hence, the correct application of the canon to the material propositions under consideration is also a presupposition of every valid conclusion, and this fact should appear in the statement of the presuppositions to every inference. Four propositions are thus presupposed by the conclusion of every deductive inference (unless it is a case of immediate inference), whether the reasoning is by means of an Aristotelian syllogism or a mathematical or logistic deduction or any other form of deductive inference. The logical independence of these four propositions is shown by the fact that none of them can be inferred from any of the others, and that the denial of any one of them does not necessitate the denial of any other. Because of the analogous character of these different forms of deduction, they may all be called syllogisms, and the term "Aristotelian syllogism" used to distinguish what has traditionally been called the "syllogism." The derivation of the word "syllogism," from the Greek term for a "collecting" of premises, also justifies us in thus extending the meaning of this word. A similar proof would show that immediate inference is analogous to the syllogism, except that it has only one premise.[1]

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[1] Mr. Russell has challenged any such analysis according to which every piece of deductive reasoning must have a definite number of premises and propositions, in *Principles of Mathematics*, sec. 39, pp. 35–36. His argument seems to be that all the propositions implied as premises to any conclusion may be simultaneously asserted as a single proposition, and that by the process of "exportation it is always possible to exhibit an implication as holding between single propositions." This process of exportation is that of changing a syllogism into an immediate inference, which change often renders the process of inference less clear, and, in the illustration that Mr. Russell gives, seems to require syllogistic demonstration of the conclusion of the syllogism (i.e., that the conclusion r does follow from the premises p and q) before it can be applied. The simultaneous assertion of several premises, if it is the stringing of them together by "and," does not change their deductively independent nature; on the other hand, if

§ 18. In the syllogistic character of all deductive demonstration, whether mathematical, logistic, or Aristotelian, together with scientific definition (to be discussed below), we have discovered the nature of complete deductive rigor. For it is only by putting a demonstration into this form that it is possible to state all the presuppositions of an argument and to show clearly and exactly how they are combined to produce the given conclusion. By so doing, the logical nature of an argument is fully enunciated, and it is completely thrown open to criticism. Hidden presuppositions, inaccurate statements, incorrect combinations are all expressed, and easily recognized by

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such a simultaneous assertion is the combination of the propositions into another unity, it is itself a syllogistic process.

On another page (p. 41), Mr. Russell states that "the rule according to which the inference proceeds is not required as a premiss," and he supports this statement by the infinite regress which would ensue if a deductive proof were offered for what Mr. Russell calls the "rule." By the term "rule" he does not mean the proposition that we have called the canon, but the proposition that would result from the combination of the canon and the validating proposition. We have recognized that in any attempt to demonstrate a validating proposition by purely deductive means an infinite regress would result, but the non-deductive nature of the proof of a validating proposition does not justify either its elimination or that of the canon. The necessity of these two formal propositions to every syllogism or immediate inference is shown by the fact that a denial of either of them, even if combined with the assertion of the other propositions of the syllogism, renders the syllogism invalid, and that no other proposition which is not equivalent to them could take their place.

Mr. Russell has taken the mathematician's practical attitude of attaining the conclusion with a minimum of effort, when he proposes to drop such a "rule" because the argument is valid without explicitly stating it; and he deliberately rejects the philosophical attitude of searching for all the presuppositions of an inference. He recognizes that there is a "rule" in every deductive inference, and that an attempt to validate this "rule" by a deductive process leads to an endless regress, so he proposes to neglect such a rule, because the lack of an explicit statement of the rule does not invalidate the reasoning! He fails to remember that rigorous demonstration ought to make explicit all that is merely implicit in a piece of reasoning, and so he sacrifices rigor to easy attainment of the conclusion. Kant was right, as against Mr. Russell, in holding that the synthetic processes of mathematics rely upon intuition, i.e., immediate description.

the discerning critic. Because of the non-deductive nature of the proof of a validating proposition, there may be an opportunity for inaccuracies to creep in through the incorrect affirmation of such propositions; but the careful critic will check over these validating propositions and thereby obviate that source of error. The complete statement of the material and formal propositions of a syllogism renders such checking very easy.

The reason that the validating proposition has been neglected by logic is probably that its statement adds little to the correctness of the argument; it is only its proof that is worth while. The omission of the validating proposition in the statement of an argument leads to no difficulty, providing that its presence is clearly understood. The same situation is found in dealing with records of observations—the correspondence of the recorded observation with the observed experience needs to be checked, and a careful scientist will repeat and check the observation; but the statement that a check has been made adds nothing of itself to the correctness of the record, although it adds much to our confidence in it. In deduction we have the immense advantage of dealing with permanently unchangeable materials, so that the correctness of a validating proposition is always open to observation. We may conclude that the nature of rigorous deductive demonstration is to be found in putting each step of a deductive argument into the form of a syllogism and thus laying it bare to criticism.

§ 19. Since strict logical rigor requires the establishment of the correctness of the validating proposition of every syllogism, it is necessary to inquire what is required for that purpose. The chief requirements are definitions of the terms and relations involved, together with definitions of the various elements required in the sort of syllogism used, in order that they may be compared with the material of the syllogism. It follows that, in stating the requirements of complete logical rigor, the problem of definition must be dealt with. The greatest difficulty here lies in the circularity of definition which appears to be inevitable. If everything must be defined in terms of something else, how can there be any starting-point; and if there is no starting-point, how can there be any such thing as sure definition? Before we meet this difficulty, it will be necessary to say something about the kinds of definition and the requirements of each kind.

The sort of definition used depends upon the purpose for which it is made. The three chief types of definitions are accordingly scientific, dictionary, and abbreviating definitions. Scientific definitions are those made for the purpose of enabling the accurate identification of an object or experience; dictionary definitions are those framed for the purpose of enabling the reader to gain an understanding of the meaning of words; abbreviating definitions are those that sum up a complex concept in a single term, so as to shorten the process of inference. The *abbreviating definition* states that one concept is equivalent to (interchangeable with) a more complex group of concepts and so may be substituted for them. Russell and Whitehead have used the abbreviating definition widely in *Principia Mathematica*. It can be used only within a deductive system and can define only concepts that are otherwise defined (or undefined).

Since *dictionary definitions* are intended merely to be helpful to readers, they need not follow the rules of scientific definition. Circularity of definition is unavoidable and unobjectionable in dictionary definitions. Since no one uses them who has not already acquired a considerable vocabulary, the essential requirement is to state the meaning of the word to be defined in terms of words that are already known, and to distinguish the word defined from other words with which it may be confused. Hence the avoidance of defining *ignotum per ignotius* and the exact commensurability of the definition with the meaning defined are the most important requirements of the dictionary definition. In fulfilling its purpose, definition by synonyms is often very helpful, and a negative definition, although not usually good practice, yet may be quite correct, if it is commensurable and enlightening.

The requirements for a *scientific definition*, which is to enable the exact identification of an object or experience, are somewhat different. A scientific definition is made for the purpose of applying it to cases or instances in order that deductions from a hypothesis may be checked with experience. Consequently, the fundamental requirements of such a definition may be summed up in the rule: Define so that a user of the definition may be able to tell exactly whether any supposed case of the term defined is or is not a case of it—if it is, that it is; and if it is not, that it is not. Unless a scientific definition indicates clearly whether any supposed case is included under the term defined, it is useless. This requirement means that, in the definition of a class, the boundaries of the class should be carefully marked out, so that it can be determined whether a supposed instance belongs to the class or not; and, in the definition of a quality, the characteristics of the quality should be clearly stated, so that it can be determined when the quality is present and when it is absent. Unless this requirement is strictly met, the definition is not suitable for use in rigorous reasoning. In meeting this requirement, it is well, in stating a definition, to consider exceptional or extreme cases, so as to be sure that the definition will not break down.

In order to meet the requirement we have stated, it is furthermore necessary, not only that the definition be exactly commensurable with the meaning in mind, but also that each term or characteristic used in the definition should likewise be scientifically defined—if a vaguely or uncertainly defined term is used in a definition, that definition itself becomes vague or uncertain. Hence, the problem of defining the ultimate terms of definition must be solved before any definition can be rigorous. It is also obvious that no term should be defined, directly or indirectly, in terms of itself. These are the general requirements for scientific definition.

There are four chief kinds of scientific definitions. Logically first, but the last to be reached in the development of science, is the *essential definition*—a definition which is so stated that all the properties of the term can be deduced from the definition itself (with the aid of other known principles, when necessary). The definition of a triangle given in geometry is an example of this sort of a definition. Such a definition presupposes that the science to which the definition belongs forms a fairly large deductive system; otherwise it would not be possible to derive the properties of the entity from its definition. Outside of conceptual sciences like mathematics, such essential definitions are as yet rare. The essential definition is historically the goal of science, not its starting-point, since the essential definition is in reality a summary of knowledge—a statement of what characteristics are logically fundamental, so that all other characteristics can be derived from them. Physics, in such a conception as the Bohr theory of the atom, is approaching an essential definition of many physical entities. In the earlier steps of science, however, essential definitions are impossible. Fortunately for us, the method of rational induction is able to gain knowledge piecemeal and is not hampered by the lack of essential definitions.

Most of the definitions in use today are *nominal definitions* —those which fulfil the general requirement of scientific definition and are stated in conceptual terms, but without enabling the deduction from the definition of all the properties of the object defined. Most of Aristotle's definitions, in terms of genus and differentia, are merely nominal definitions; although he required the essence to be stated, he did not even define "essence" in such a way that it could always be determined whether the essence had been given or not. Since all that is necessary for a nominal definition is to give a sufficient number of indications to determine *uniquely* the class or quality under consideration, it is quite possible that there may be a large number of nominal definitions of the same term, for each unique property or group of properties, even if it is only possession of a particular space or time, is sufficient to constitute a nominal definition. On the other hand, the number of essential definitions of a given concept is very small, although we cannot say that always only one is possible for each term, since there are even alternative methods of constructing such a figure as an ellipse, and the choice of any one construction to define the figure is therefore arbitrary.

In scientific definition, the avoidance of defining *ignotum per ignotius* is unnecessary and often impossible. Instead of this principle, it is required that a definition should be stated only in terms of what has been previously defined; thus giving rise to a hierarchy of definitions with proximate and ultimate definitions, the proximate definitions resting upon ultimate definitions. It has been contended that ultimates are indefinable; but, if our account of definition is correct, the admission of ultimate indefinables necessarily implies that there are no scientific definitions and that logical rigor is impossible. We admit that, in the present state of science, many terms are difficult to define, and that many have not been scientifically defined; but the fact that a problem has not been solved is no proof that it cannot be solved. We must inquire how ultimates, including *summa genera*, can be defined.

It is necessary, first, to distinguish between mathematical indefinables and other ultimates of definition. We shall see later that the indefinable of pure mathematics is not strictly an indefinable but a term that is purposely left partly undefined for the sake of greater generality of application.[1] We shall see further that the entities of applied mathematics do not exist in the sense that physical objects exist, and that they are limits to which physical objects approach, so that there are properly no cases of straight lines, etc.[2] Definitions of limits may be difficult, but they are by no means impossible. The mathematical indefinable is a different sort of entity from the ultimates of definition, and cannot be adduced to prove that there are real indefinables.

If there were such things as real indefinables, they would necessarily be indefinite, so vague that they could not have any clear meaning. There are undoubtedly such concepts in many minds. Nevertheless, it is no excuse for nebulous reasoning to say that it is so nebulous that it cannot be stated clearly! The meaning of even a *summum genus* may be made sharp and clear, and, if the meaning is clear, it should be statable or indicable in some way. The doctrine of logical indefinables, except in mathematics, has done a great deal of harm, especially to the thought of those who have accepted it whole-heartedly, for it has too often been an excuse for sloppy thinking. Leaving such terms as "God" or "existence" without even a nominal definition, under the plea that they are indefinables, has brought ambiguity and self-contradiction into many a philosophy.

Many ultimate units of experience can be defined in terms of the conditions under which such an experience occurs. For example, "red" may be defined as the experience that a normal person has when his retina is stimulated by light waves 760 $\mu\mu$ to 640 $\mu\mu$ in length. Ambiguity in the meaning of such a definition (for we always experience many other things at the same time that we see red) can be prevented by repeating the experience with the other factors (except the specified conditions) varied, then taking the experience common to those cases—a logical process similar to Mill's method of agreement. Such a definition is not purely conceptual in its nature, since it relies upon experience; it can, however, be made as sharp and clear as necessary. Since it defines a concept in terms of the conditions under which a certain experience occurs, it may be called a *causal definition.* By the use of such causal definitions, most, if not all, of the ultimate psychological units of experience can be defined.

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[1] Cf. chap. xii, § 2.

[2] Cf. chap. xii, § 5.

While the causal definition enables us to define many supposed indefinables, yet it does not solve the problem of ultimate definition, since, as long as any conceptual factor other than the term to be defined enters into the definition, that conceptual factor itself still requires definition. We must seek a wordless definition to define the ultimate ultimates.

Such a definition can be secured through action, especially pointing. Pronouncing the name of the term to be defined, and at the same time pointing at sufficiently varied sets of situations, so that the only thing common to all the situations is the meaning which is being defined, will, especially if repeated, eventually bring to the mind of a person who observes the situations carefully a hypothesis as to the meaning intended, which he can verify by comparing it with the varied situations that are presented. Thus he can finally gain a clear and exact notion of the meaning of the term that is pronounced. Such a definition may be called a *denotative definition.*

A foreigner among a people of whose language he cannot understand a single word usually arrives in this fashion at the meanings of words; children acquire their first vocabulary by the use of this method. If it is objected that this view assigns to children great logical power (involving, as it does, a logical process similar to the method of agreement), it must be remembered that the logical ability of children is often greatly underestimated, because they use logic so intermittedly. While the rigorous use of the denotative definition is difficult, yet it is not impossible, especially when we remember that only one term may need to be defined without the use of any concepts, and that, when that one term is acquired, it can be used to aid in defining the second term, so that, after the (logically) first denotative definition, subsequent definitions may be made by a combination of denotative and conceptual means. It should be remembered that even if the (logically) first definition is not acquired quite correctly, further experience with the use of that term will finally correct it, so that several denotative definitions may be acquired simultaneously. In this way, children learn that "dog" does not apply to all furred animals, but only to one species. Even relations, such as "and," "for," "which," may be defined by this method.

The use of denotative definitions will enable the definition of any and all supposed "indefinables," since all that is necessary to give a denotative definition is (1) a clear and exact notion of the meaning of the term in the mind of one person, (2) situations that contain examples of the term in mind, and (3) an attentive and intelligent listener. The first of these prerequisites is the hardest of all to secure, although the second may make difficulty. The necessity of the second requirement precludes the possibility of ever defining a color to a man born blind, or of defining in this way any notion that is never experienced. If God is absolutely beyond experience, as in negative theology, then he cannot be defined denotatively. It may be possible, however, to give a definition of something that never has or can be experienced when that concept is composed of elements which themselves come within experience. Such a definition must then be a conceptual one. The ultimates of physics, such as mass, force, etc., are defined in this manner.

As a matter of practice, the purely denotative definition is not often given. Except in personal conversation, its strict use is impossible. Because of this difficulty, the procedure in scientific definition is usually to accept a group of common terms having definitely understood meanings, and to build all definitions out of that group of terms. By the use of causal definitions, this agreed-upon vocabulary may be made very small. Then the denotative definition is resorted to only when the meaning of one of this small group of terms is found to need definition. These terms comprise roughly the untechnical and commonly used words in their usual meanings. As soon as a term becomes technical or takes on an unusual meaning, it should be defined. The important fact is the realization that all terms can be defined, if necessary, for thereby we escape from the damaging position of leaving fundamental terms undefined, whereby rigorous reasoning is made impossible. The reason that failure to define these common terms leads to no lack of rigor is that there is actually agreement as to their meaning, and that they can be defined, if it should appear that the required agreement is lacking.

One curious result of this procedure is that philosophy is put at a disadvantage in comparison with other sciences, for philosophy finds that it needs to use many commonly used terms, such as idea, cause, knowledge, with technical meanings, which may be slightly different from their usual meanings; consequently philosophy finds it necessary to define many terms that other sciences, because of their different subject matter, can leave untouched. That circumstance does not mean that philosophy needs to define *all* terms, beginning at the beginning, any more than any other science, but merely that it has a more difficult task, since philosophy deals with more fundamental problems than any other science.

The denotative definition is not a chimera—denotative definitions are commonly given (though usually without sufficient variation of circumstances to make them quite unambiguous) when a language is taught by the "direct" method. In such cases the time and energy needed to give denotative definitions are justified by the increased efficiency thereby secured in learning the new language. We also go through a process whose logic is very much like that of the denotative definition whenever we wish to check the meaning of a term which a particular author uses—we gather together many different cases of the use of that term, compare them, and abstract the common element, if any. In case the author has used the term in contradictory senses, there may be no common element. Likewise, usage is continually defining common words, since, if any term is used in a sufficiently varied number of circumstances, only one meaning will be found to fit that term, and so its meaning is fixed. Lexicographers take advantage of this fact to fix the meaning of words in dead languages or authors.

The realization of the difference in purpose and in requirements for a dictionary, an abbreviating and a scientific definition, and the difference in nature between the essential definition, the nominal definition, the causal definition, and the denotative definition, thus enables us to show the possibility of rigorously defining every term. We then see the falsity of the doctrine that some terms are necessarily indefinable, and are able to avoid the resulting ambiguity and confusion in fundamental matters that philosophy has been plunged into.

§ 20. In the syllogism there is to be found a powerful instrument for discovering the ultimate presuppositions of any deductive argument, thus enabling the enunciation of *all* the primitive propositions of any deductive or mathematical system. The statement of the five propositions of a syllogism or the four propositions of an immediate inference gives a complete list of the immediate presuppositions to any given conclusion;

we have only to seek for a full statement of the prosyllogism that demonstrates the truth of each of the material and formal propositions of our original syllogism, and for the prosyllogisms to each of the propositions thus found, in order to arrive at a complete list of the primitive propositions of the demonstration. Thus the process of throwing a demonstration into completely syllogistic form is at the same time a discovery of all of its presuppositions. Such a procedure would not necessarily assure us that we had decomposed all of the presupposed propositions into their simplest form, for there might be a prosyllogism that we did not notice, belonging to one of our apparently primitive propositions; in that case the syllogistic statement of an argument would be no guaranty that a statement of the ultimate or simplest presuppositions has been made; such a procedure would, however, at least assure us that we have omitted none of these ultimate presuppositions, since they would necessarily be implicit in the list of presuppositions which we have discovered, and consequently the task of enunciating them would be rendered very much easier. The syllogism is thus an extremely powerful instrument of logical analysis.

When we bear in mind the syllogistic nature of all deduction, we realize that all existing lists of primitive propositions for mathematical or logistic systems are incomplete, since they always omit many of the important canons of deduction, if not other propositions, too. This fact is true even of *Principia Mathematica*, in spite of the explicit assertion of the authors that they have made all their assumptions explicit.[1] Hilbert's axiom of completeness[2] is likewise faulty, but in a sense different from that in which he meant it. He and other mathematicians have usually stated correctly the material presuppositions of their systems, but have failed to state the formal presuppositions of deduction.[1] In some cases,[2] mathematicians have recognized that the primitive propositions they have stated are combined in accordance with the laws of formal logic. It would be unfair to expect the mathematician to investigate these laws of formal logic; nevertheless, an attempt to put mathematical proofs into the forms of logic would soon have shown the inadequacy of the existing logic for that task. Logicians must again study mathematics, as they did in the days of Plato.

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[1] Cf. Appendix to this chapter.

[2] D. Hilbert, *The Foundations of Geometry*, p. 25.

§ 21. It may be worth while here to consider some consequences for logic and metaphysics of the foregoing analysis. In the first place, the hope, which Socrates expressed in the *Republic*, of deducing all knowledge from *one* proposition is obviously futile. Aristotle showed the bootlessness of this hope[3], but it has reappeared again and again in the history of philosophy, most recently in Cassirer's ideal of a "scientific concept," which has "concrete universality" because from it can be deduced the special cases that it subsumes.[1] The very illustration which he gives shows that such a "scientific concept" is impossible. He uses as an example the general formula of a curve of the second order, from which can be deduced the special geometrical forms of the circle, the ellipse, etc. That process of derivation, however, as he himself states, is by the introduction of a certain parameter, which is allowed to vary. Thus the general formula is the major premise of the deduction, but before the circle can be derived it is necessary to introduce a parameter, which is the minor premise; if two premises are combined, that must be done in accordance with a rule, and the rule must be observed to be correctly applied to the case—in this way *four* propositions are inevitably implied in the deduction of a circle from the general equation of a curve of the second order, so at least *four* concepts are shown to be needed, not only one! Each different curve deduced from the general formula would require at least two propositions that are different from any other, a different parameter and a different validating proposition. The ideal of deduction from only one proposition is chimerical.

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[1] Because of (1) the non-deductive character of the proof of a validating proposition, and (2) because no two validating propositions are identical, but each one is a singular proposition that applies only to "this syllogism," and (3) because the validating proposition merely guarantees the formal validity of the reasoning, the omission of the validating propositions from the list of the presupposed propositions to a mathematical system like that of a geometry or *Principia Mathematica* would be no loss, provided that these propositions were clearly understood, and that it was clearly realized that their proof is by a non-deductive method. The material necessary for the proof of these validating propositions, the definitions of different types of syllogisms, of the types of propositions used in each kind of syllogism, the types of terms and relations, etc., in each kind of syllogism, etc., all of which are now either omitted or tucked away in the prolegomena, should be stated explicitly. It is probably the dim realization of the necessity for stating the characteristics of different kinds of propositions that induced the authors of *Principia Mathematica* to spend so much time upon a prolegomena which they say is not required in the demonstration, but which is now shown to be essential.

[2] Veblen, in *Transactions of the American Mathematical Society* V, (1904), 346 ff.

[3] *Posterior Analytics* I. iii. 4.

§ 22. Purely deductive reasoning is furthermore powerless to draw a single conclusion with certainty. It has long been known that deduction is unable to provide its own first premises; but it has not often been recognized that even when a set of primitive premises is provided, nothing can be drawn from them with certainty by deduction alone, because each cogent act of deduction requires the (implicit or explicit) proof and assertion of a validating proposition, which can refer to "this syllogism" or "this immediate inference" alone. The ultimate reason for the necessity of a validating proposition lies in the fact that the conclusion of a syllogism does not merely assert the *occurrence* of a particular fact or relation, but it asserts that as a consequence of our original presuppositions we *know* that such a fact or relation *must necessarily* occur; hence the drawing of a conclusion implies, not merely the premises, but the fact that the conclusion is *properly* drawn, which presupposition is stated in what we have called the validating proposition.

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[1] *Substance and Function,* I, 1, chap. iii.

Some logicians have failed to realize the helplessness of purely deductive reasoning because of the necessary presence of a validating proposition, which cannot be established by deductive processes. So we find them living in realms of eternal truth and denying cogency to any non-deductive reasoning! Unless some non-deductive process can give just as much certainty as deductive processes, deduction itself cannot give certainty. Since a validating proposition is always a singular proposition, which refers to "this syllogism" and no other, a deductive proof of such a proposition must depend upon the ultimate assumption of some proposition concerning "this syllogism." Unless a primitive proposition referring to each syllogism is included in the primal assumptions of the system, no validating proposition could be deduced, for a singular proposition cannot be deduced from a universal proposition, except by the aid of another singular proposition. Furthermore, a validating proposition would be required for the syllogism that establishes the first validating proposition, and the second one would require a third, and so there would be another infinite regress! In reality, the proof of a validating proposition is not by a deductive process, but by a non-deductive process of immediate observation. The necessity of establishing the correctness of a validating proposition in each syllogism of a rigorous proof makes *purely* deductive demonstration impossible.

§ 23. Our analysis shows us, in the third place, the hopelessness of the ideal of modern rationalism, that of founding demonstration upon a *small* number of self-evident or otherwise established postulates, and of deriving a philosophic system by deduction alone. A pure rationalism of this sort would have to list all of its validating propositions among the presuppositions of the system, the number of which would be equal to the number of syllogisms and immediate inferences in the system! The more inclusive and well worked-out the system is, the larger the number of primitive propositions! Yet if the validating propositions were omitted, the system could not have certainty or necessity. A rationalism that depends solely upon deduction from a small number of propositions is impossible; yet, if validating propositions can be established by a non-deductive means, why should philosophy depend exclusively upon deduction?

It is furthermore to be noted that the syllogism, as we have analyzed it, is not an instrument of discovery, but merely an instrument of criticism. The usual psychological procedure of thought is to arrive at the conclusion first, by an act of intuition, and then afterward, often by laborious thought, to analyze out the steps by which the conclusion is demonstrated. Recognition of the purely critical function of the syllogism will go far to invalidate the traditional objections to it, such as those that it yields nothing new, that it is useless to discover truth, and that it does not represent the course of thought in reasoning. The function of the syllogism is merely to test an inference that is already drawn, to determine its validity, or to indicate what propositions are needed to establish a conclusion whose ground is already partially given. It has little to do with psychological processes of thought, but much to do with logical processes of proof.

§ 24. It may help in establishing our thesis to ask the question, Why is it that mathematics, which is such an ancient and highly developed deductive system, and logic, which deals expressly with inference, should have failed to discover the syllogistic nature of all deduction or the fact that each syllogism contains exactly five propositions? In reality, the greater part of what we have stated has been implicitly recognized. Aristotle stated the canon of all syllogistic inference as he understood it, in the words which were later formulated in the *dictum de omni et nullo*. Mathematicians have stated a great many of their canons—the first eight of Euclid's axioms are usually used as canons of inference; the "associative" and "commutative laws" are canons. Logicians have been far more lax than mathematicians in stating the canons that they were using. The necessity that reasoning should be formally correct has always been recognized, and, whenever a deduction has been checked for its correctness, the validating proposition has been implicitly asserted. At various times some mathematicians have realized the syllogistic nature of mathematical deduction, but the attempt to secure mathematical elegance and conciseness, the great complexity of mathematical inference, and the variety of canons used, prevented the wide recognition of the syllogistic nature of mathematics.

On the part of philosophers and logicians, various reasons have hindered the recognition of the true nature of deduction. Some philosophers have made capital out of the unexplained: implication was considered to be mysterious and unanalyzable; necessity was an ultimate mystery, due to the nature of Absolute Mind. The eighteenth-century philosophers thought that mathematical inference was different in kind from syllogistic (Aristotelian) inference, because mathematics brings novelty and discovers truth, whereas syllogism (Aristotelian inference) does neither. The explanation of that appearance is simple. The mathematics of the time, especially geometry, did not state all of its presuppositions, as mathematicians are now coming more and more to recognize; consequently, its discoveries were really new and different from their partially stated premises, as well as having the relative newness which every conclusion has as compared with its premises; on the other hand, logic attempted to state its (material) presuppositions, and so it was apparent that the conclusion was no more than a result of the combination of what was in the premises. Again, it is probable that some logicians were frightened away from the discovery of the validating proposition (and also of the canon) just as was Mr. Russell,[1] by the intuitive preception that no validating proposition can be proved by purely deductive means, and that therefore the "necessity" of pure deduction would disappear.

§ 25. The most potent cause for the failure of science to perceive the true nature of deductive method is the different attitudes taken toward deductive reasoning in what we may call the practical and the philosophical moods. In argumentation, the purpose of the practical man is to convince his hearer or reader of the correctness of his reasoning, and the more quickly he does so, the better. That is why enthymemes are so much more common in ordinary argumentation than are syllogisms. We pass from the statement that "Socrates is a man" directly to the conclusion, "Socrates is mortal," without stopping to state the major premise; and, if our auditors accept the inference, there is no need to mention the major premise anymore than the canon or the validating proposition. Indeed, it would usually be positively harmful to do so, for a careful statement of these propositions would take the attention of the listener away from the point at issue, and actually detract from the cogency of the argument—the listener would be thinking about the possibility of challenging the expressed major premise, or he would be disgusted at our pedantry, instead of admitting the point at issue. This effect upon the listener is the reason that an enthymeme or a sorites is so much more effective than the same reasoning set out in syllogistic form—the distraction brought about by carefully setting out an enthymeme or a sorites in syllogistic form withdraws attention from the conclusion to be established, and the result is that the degree of conviction engendered in the hearer may even be lessened.

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[1] *Principles of Mathematics*, p. 41. Mr. Russell sees that the proof of a "rule" (canon plus validating proposition) will lead to an infinite regress. He asserts that "any proposition warranted by a rule of inference does actually hold, and is not merely implied by the rule." Since the implication is really "there" independently of proof, there is no necessity of proving that it is "there," and so the proof of the validity of the inference may be neglected!

In mathematics, a similar condition obtains. The mathematician is eager to establish his theorem, and the fewer words and symbols he uses to do it, the better. Mathematical proofs are complex and long; if they were fully stated, it would be difficult to remember them; consequently they are compressed as much as possible, and only just as little of the proof is given as will be necessary to indicate to an informed mind what the course of the demonstration is. Mathematical elegance consists largely in just this conciseness and statement of only the pertinent parts of the argument, so that a good mathematical demonstration may be compared to an epigram in its brevity and strikingness. Hence a well-stated mathematical demonstration is much easier to grasp and remember than a poorly stated one. Then the mathematical layman who wishes to follow through the proof is left to puzzle out the intervening steps of the demonstration, which the rapidly moving mind of the mathematician has not thought it necessary to supply. For similar reasons, the philosopher rarely stops to state his reasoning in formal fashion, even when his argument is a purely deductive one; he disdains such pedantry, and rushes on from one statement to another as rapidly as he thinks the minds of his readers can follow him. The attainment of the goal with as little effort as possible is the aim of a reasoner who takes the practical attitude. Such a process, even though it may not lead to as great accuracy or correctness as syllogistic reasoning, is certainly more persuasive.

The philosophical or theoretical attitude toward inference is just the opposite of the one we have been describing. The man who takes this attitude is not interested in hastening on to the conclusion, because he already knows what it is; instead, he is interested in testing the validity of the inference and in discovering what are the presuppositions of the argument, in order that he may evaluate the inference thereby; for that purpose he finds it necessary to supply the missing premises of an enthymeme or a sorites, and to state the formal propositions of each inference. In this search for logical rigor, the canon of the syllogism and the validating proposition become important, for, without them, all the presuppositions of the argument cannot be stated. It is this theoretical attitude, and not the practical attitude, that leads to the analysis of deduction we have made.

While the discussion of the fundamental principles of any system is a philosophic problem, yet the Aristotelian syllogism, which was the peculiar possession of philosophy, is so restricted in its application that it does not yield an extended system of inferences without pedantic cramping of thought into its narrow forms. Philosophers, in developing their systems, used the types of inference found in common speech, which are often more closely analogous to mathematical inference than to the Aristotelian syllogism. Consequently, the philosophical discipline of logic had no system to analyze, while the practical discipline of mathematics had a well-developed system, but few men interested in the philosophical problem of analyzing the nature and bases of inference. Yet mathematicians have not been without a philosophical interest in the nature of their science; we are better acquainted with some of the canons of inference used in mathematics, such as "Things equal to the same thing are equal to one another," or "If equals are added to equals, the sums are equal," than we are with their analogues in Aristotelian inference. But there has been no general method of discovering the presuppositions of an argument; that method is first presented in our analysis of deduction; consequently the discovery of the presuppositions of mathematics had to depend on those mathematicians who were favored with intuitive insight, instead of being methodically determined in accordance with syllogistic analysis. The difference between the practical attitude characteristic of the mathematician and the theoretical attitude characteristic of the logician is largely responsible for the neglect of rigorous syllogistic analysis in deduction.

§ 26. Geometry has suffered most from a lack of adequate analysis. It has the doubtful advantage of being able to picture its inferences in sensuous form. The result is that many propositions can be judged to be true from the figures; consequently, the mathematician, in his eagerness to present a mathematically concise demonstration, is not always careful to examine and see whether or not his stated premises justify his inferences. In particular, geometers have usually failed to demonstrate the construction of the figure that is used in the demonstrations of a theorem. Thereby the argument ceases to be rigorous. With the figure drawn and before one, the necessity for a demonstration of the properties of the construction does not appear obvious. But, as J. W. Young has shown, a slight inaccuracy in the drawing of a figure may throw the whole demonstration askew and lead to self-contradiction.[1] Were the properties of every figure demonstrated before the figure is used, such gross mistakes would be impossible. It is perhaps not too much to venture the statement that if the properties of geometrical figures were demonstrated in rigorous syllogistic form and if rigorous syllogistic analysis were carried throughout geometry, it would be found necessary to change and modify a very great deal, especially in the beginning parts of the science, and the fundamental assumptions of geometry would be found to be larger in number than has been realized.

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[1] *Fundamental Concepts of Algebra and Geometry*, p. 143; he thus demonstrates the theorem that "any triangle is isosceles"!

The practical attitude toward inference, while it gives elegance to a demonstration and adds to its persuasiveness, yet leads to the employment of enthymemes and the omission of steps in the reasoning. An enthymeme may be just as correct as a fully expressed syllogism, yet it is a common experience to find in the unexpressed proposition of an enthymeme the weakest or fallacious part of the reasoning. So the practical attitude toward inference has sometimes led mathematicians into error as well as to elegance, often because of the superior persuasiveness of such an argument.

§ 27. To sum up this part of the chapter: We have found that there are analogues of the Aristotelian syllogism in other types of deductive inference, which analogues may well be called syllogisms, and we have also found that the traditional immediate inference has likewise its analogues in mathematical and logistic inference. Had we pursued our investigations further, we should also have found analogues of the enthymeme, sorites, and other forms of Aristotelian inference. We found that each syllogism, of whatever sort, contains a definite number of propositions, namely, five, which are discovered by the process of searching for all the deductively independent propositions whose denial would invalidate the inference. These five are: the two material propositions or premises, which may be distinguished as major and minor, or undistinguished in importance and called merely first and second premises; the two formal propositions, the canon of combination and the validating proposition; and the conclusion. A case of immediate inference was found to contain one less premise, but otherwise to be analogous to the syllogism; it was found to be a means of abbreviating certain kinds of commonly used syllogistic inferences.

In addition, we found that the syllogism and immediate inference (or transformation), in fact, all deductive inference, contain a non-deductive element. Hence we found that nothing can be established by deduction alone, and that unless certainty can be had by some non-deductive process, no certainty can be obtained by deductive means. Syllogisms may be classified in accordance with their types of canons; all deductive reasoning can be reduced to syllogisms and immediate inferences. In this syllogistic analysis of deductive argument and by means of scientific definition, we discovered the instrument whereby complete logical rigor could be obtained, and a means of discovering all the presuppositions of any deductive argument. Our analysis also showed us the futility of deducing all knowledge from one proposition, the impossibility of establishing a single inference by deduction alone, and the futility of the ideal of demonstrating a system of knowledge from a small number of postulates.

In dealing with the problem of the indefinable, we found that definitions are of three kinds: abbreviating, dictionary, and scientific definitions, and we stated the requirements for each one. The last kind possesses four species: essential, nominal, causal, and denotative definitions, by the use of which even supposed indefinables may be defined.

In our discussion of the reasons why our analysis had not been previously recognized, we saw that it has often been implicitly recognized, but that the practical attitude toward inference and the demand for elegance in mathematical demonstration have prevented its explicit recognition, and that this same practical attitude has led mathematics to fail in recognizing all its presuppositions, and sometimes has prevented it from making its proofs rigorously exact. With the analysis, provided by the syllogism, of the presuppositions for any inference, it is to be hoped that the deductive sciences will proceed to state in full *all* their presuppositions, and not only the important or striking ones.

§ 28. Our analysis of the syllogism enables us to answer the objections which have been made to it, and to show that most of them are due to misunderstanding or captiousness. It is chiefly due to a misunderstanding of the proper function of logic and of the syllogism that so many modern philosophers, beginning with Descartes and Bacon, have criticized the Aristotelian logic. They supposed that the function of logic is to discover new knowledge, that logic is the science and art of reasoning, that it should give truth; while they found that logic, especially the syllogism, cannot discover anything new, but can only set out those truths already known. They also found that the procedure of the syllogism is a *petitio principii;* that the natural procedure in argument is not syllogistic; and that syllogism cannot convince absolutely or give truth, because it can never prove its major premises. They therefore concluded that syllogistic logic is useless. These objections, which constitute the staple of the traditional attacks upon deductive logic and the Aristotelian syllogism, are due to a misunderstanding of the nature of logic; philosophers have expected logic to do that for which it is unfitted, and then have become disappointed in logic! The better procedure would have been first to have discovered the proper function of logic before criticizing her.

Against the objection that the syllogism does not bring forth anything new, we need only restate the account of the syllogism which we have already made. Its function is not discovery, but criticism; its use is to test an inference that has been already drawn: to determine its validity and to indicate what presuppositions are implied by the conclusion whose ground is partially given. If criticism is useless, the syllogism is useless; but unless criticism of our wholly or partially accepted beliefs is worthless, the syllogism is a highly valuable instrument. Criticism does not furnish us with anything new, so we ought not to expect the syllogism to provide us with conclusions which we do not already know.[1] This objection misunderstands the function of the syllogism.

The second objection, that the syllogism does not represent the course of thought[2] must be admitted, but the realization that the syllogism is a mode of criticism, not of discovery, robs it of its force. The usual course of thought in discovery, as we have seen, is by a flash of intuition. Sometimes thought does proceed in a manner that is expressed in the Aristotelian or other forms of syllogism, and so we may call the syllogisms forms of inference; but more often the course of thought is lawless, a burst of insight that sweeps aside all logic and goes straight to its conclusion. Such intuitions are merely nonlogical; the psychologist may study and describe them; but they are not amenable to rules of procedure. When we understand that the function of syllogism is criticism only, these considerations lose their force. This understanding also takes the sting out of the statement that the syllogism depends on a vulturine analysis of defunct thought; hence what logic analyzes cannot be real, and what is real it refuses to analyze; consequently the syllogism doubly falsifies thought.[3] The function of the syllogism is the criticism of knowledge; knowledge is never the thing that is known, it is only *about* the thing that is known: hence knowledge, because it is the result of thought, may be called "defunct thought" and "unreal"; but the hurling of such adjectival descriptions of knowledge at the syllogism is no real criticism of it. Mr. Schiller, like Bradley, seems to desire a psychological account of thought, and to be dissatisfied with logic because it refuses to be psychology!

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[1] Cf. Bradley, *Logic*, p. 226. He says that a conclusion "must convey some piece of information, and must tell us something else than the truths it depends upon. We have no inference at all, we have simply a frivolous show and pretence, if taking something we already know we assert the whole of this once more, and then say, 'I have reasoned and got to a conclusion.'"

[2] In the quotation in the previous note, it is the course of thought that Bradley means by "inference." As such, he was interested in the psychological investigation of thought, rather than the logical criticism of thought. This confusion between psychology and logic runs through the logical studies of the neo-Hegelian school, to the detriment of much that would otherwise be extremely valuable.

[3] F. C. S. Schiller, *Formal Logic*, chap. xvi, sec. 5.

The third objection, that much reasoning cannot be put into the form of syllogism without straining, must be admitted to be a valid objection, as long as the term "syllogism" is restricted to the Aristotelian type of argument. But once we extend the meaning of the word to include all forms of deductive reasoning which are accepted and widely used, that objection loses its force. The Aristotelian syllogism is then seen to be only one form among many; the syllogism of substitution of values is probably used more often than it, and the syllogisms of substitution of equals, etc., are also common, both in mathematics and in ordinary argumentation. Logic should analyze these and other types of syllogisms, as well as the many types of immediate inference, and so systematize and criticize the processes of deduction. It must also be recognized that only deductive reasoning can be put into syllogistic form, and that much reasoning is inductive in character. The syllogism is restricted to deductive reasoning.

The objection that the syllogism is not used in science and that it is not a natural mode of procedure in argument is also futile. Science, especially mathematics, has not used the syllogism because it was too greatly influenced by the practical attitude, and took the philosophical attitude too rarely. We have seen that the result has often been inaccuracy and failure to recognize adequately what are the presuppositions of an argument. But the procedure of mathematics is implicitly syllogistic, in the wider sense of the word; all deduction can be reduced to the syllogism and immediate inference. Other sciences do not seem to use the syllogism because their procedure is chiefly inductive; but we shall see that deduction, and therefore the syllogism, is an essential part of induction. Forensic and other argumentation is precisely similar in its nature. The lawyer of whom Bosanquet speaks,[1] who began by an exposition of the facts of the case instead of syllogisms, was probably building up an inductive argument. Most criminal cases involve inductive arguments—starting from the known facts that the accused has been seen in a certain place, possessed a certain sum of money, etc., the most "natural" procedure is to show that precisely these actions would result from the hypothesis that he committed the crime (i.e., that they can be deduced from the hypothesis), and that unless he had done so, he would not have done these things—a use of inductive reasoning. It is no objection to the syllogism or deduction that it is not induction!

The other important objection, that the syllogism involves a *petitio*, needs more careful examination. When the definition of a *petitio principii* is taken to be that the conclusion of a deductive inference is involved in the original premises, such a *petitio* constitutes the essential characteristic of a valid deduction, and the guaranty that the conclusion is derived from those premises and no others. For if the conclusion of a syllogism introduces something new, something which is not implied in the premises, that conclusion is plainly an invalid deduction from those premises, and the syllogism is incorrect. But when the conclusion merely gives a new organization to what is found in the premises, then we have a trustworthy and valid syllogism. We have seen that the reason this characteristic of all deduction has not been more widely recognized is simply that mathematical and other deductive arguments have rarely stated all their presuppositions, and that when all the presuppositions of a deduction are stated, this sort of a *petitio* is clearly seen.[1]

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[1] *Implication*, pp. 105 and 113.

It is sometimes supposed that the only sort of inductive proof for the premises of a syllogism is by an enumeration of instances (i.e., an empirical induction), and the deduction merely draws out one of the instances of the generalization that result from that enumeration; if this procedure is not for the purpose of testing a hypothesis (in which case it would be unobjectionable), but for the purpose of asserting a new fact, there is a *petitio* of a different sort, which is a genuine fallacy (inasmuch as empirical induction cannot be certain, and hence no deduction from it can claim cogency). But when rational induction is recognized to be fundamentally different from empirical induction, and to involve no enumeration of instances, deduction is no longer a fallacious *petitio*. In particular, when deduction is used to develop the consequences of a hypothesis, so that the reasoning is: If the hypothesis is true, then *A*, *B*, *C* must follow, there is no fallacy, because what is *sought* is an elaboration of what is involved in the original hypothesis, and there can be no logical objection to obtaining precisely that. This sort of reasoning does not constitute a fallacy; if the nature of *petitio* were what we have stated, *petitio* would not be classed as a fallacy at all!

On the other hand, if *petitio principii* is defined as "the fallacy of founding a conclusion on a basis that as much needs to be proved as the conclusion itself,"[2] *petitio* is a characteristic of all pure deduction, since deduction can never prove its own premises. The fallacy consists in the failure to carry the process of proof far enough—a common omission in ordinary argument. Where deduction is combined with induction, so that the foundations of the deduction are established securely, this fallacy does not occur. Until the theory of induction is settled, there can be no secure logical theory of deduction.

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[1] Cf. § 24.

[2] H. W. Fowler, *A Dictionary of Modern English Usage*, p. 614, under *petitio principii*.

It is evident that what is needed to clear our thinking on the matter of *petitio* and the syllogism is to redefine that fallacy in such a manner that these unobjectionable *petitios* should not be included as fallacies. We suggest that something like the following may be found suitable: *Petitio principii* is a fallacy, found only in deductive reasoning, which consists either in the failure to establish adequately the truth of the premises upon which a deductive argument is founded, or in using the conclusion that it is desired to draw or some consequence of it as one of the proximate or ultimate premises. Such a *petitio* is not characteristic of the syllogism as such.

§ 29. There have been many other objections to the syllogism, though none so important or trenchant as those we have considered. They may all be met by understanding that the function of the syllogism and of logic is not to guide thought to valuable results, but to criticize the results of thought. Logic is not, as Mill says, "the analysis of the mental process which takes place whenever we reason . . . . and the rules, grounded on that analysis, for conducting the process correctly,"[1] but the methodical criticism of the results of thinking which pretend to truth. Logic is not an organon but a canon. Logic is the science of testing and proving propositions, including all that is involved in so doing, the description of types of propositions, terms, etc.; it is not the art of reasoning or inferring. Inference as a process is psychological and outside the realm of logic. Only confusion can come from the attempt of logic to deal with the procedure of thought at the same time that it attempts a criticism of thought.

§ 30. The results of deductive inference, if they are numerous and homogeneous, form a system. There are geometrical systems, algebraic systems, mathematical systems; Euclidean (parabolic) geometry may be called one system, elliptic geometry another system, and hyperbolic geometry a third. Again, *Principia Mathematica*, when completed, will include the foundations of all mathematics and deductive logic; hence the whole of deductive logic and mathematics may implicitly be considered as one system. The term "system" is thus relative, and we may speak of the deductions from any hypothesis, provided they are varied, as forming a system, just as we may say that the universe may be (ideally) a system. Deduction thus shows a power of grouping together diverse conclusions as results of one set of presuppositions; the systematic nature of mathematics and of science is due to their use of deduction as the method of elaborating the fundamental hypotheses of mathematics or of natural science. Strictly speaking, it is not the hypothesis or the set of primitive propositions that relates and explains diverse phenomena, but the deduction from that hypothesis. Deduction thus shows its importance for science.

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[1] *Logic*, Introduction, sec. 2.

This system-making characteristic of deduction has been emphasized, especially by English idealism; the creation of a system has been made the criterion of truth, a theory that we have seen to be essentially faulty.[1] It is important to remember that a system or, for that matter, any deduction cannot by itself attain to knowledge. The whole system is merely the development of what is implicitly implied by the original premises, and knowledge of its truth depends upon knowledge of the truth of its premises. If a premise is false, the consequences which are validly deduced from that premise cannot be known to be true by such a deduction. This characteristic of a system, that it depends wholly upon its premises, is necessary in order that deduction may fulfil its function—that of elaborating the consequences of a hypothesis, so that the hypothesis may be tested by its consequences. Hence deduction deduces false consequences from false premises, as well as true consequences from true premises, in order that false hypotheses may be disproved and true hypotheses established.

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[1] Cf. chap. ii, § 5.

We must further remember that among the presuppositions of every system, whether mathematical or otherwise, are not only the particular premises of that system but also the rules of logic. In our analysis of the syllogism, we showed that the canon of inference is an essential presupposition of the conclusion, and so demonstrated that the rules of formal logic are part of every system. In part, these rules are enumerated in mathematical systems among the axioms, but only in part; Aristotelian inference, especially the hypothetical syllogism, is used widely; furthermore, the proof of the validating proposition requires far more than the statement of the canon, it requires the statement of the characteristics of a valid syllogism of that type, and consequently an analysis of the types of terms, relations, etc., found in that sort of inference. Thus logic may be said to be among the presuppositions of every system, and, in a sense, logic binds them together. But the rules of logic are only part of the primitive premises of such systems; in addition, other material premises, which are individually characteristic of each system, are necessary. The fact that logic binds systems together is not because of the individual characteristics of particular systems; it is merely because these systems constitute knowledge. As such, the systematic character of science may be considered as due, not to the intrinsic characteristics of that science, but to the fact that it has become part of human knowledge.

Here, again, the emphasis of idealism upon systems is a part of its emphasis upon the fact that all that is known is knowledge. The concrete universal of modern absolute idealism is said to be the Unity that lies back of everything else and implies everything else—it is merely the mythical and undiscovered great major premise of deduction from which all judgments may be derived, or the hypostatized system of all judgments contained in the system— as such, *Principia Mathematica* might well be an embodiment in miniature of the sort of thing that the concrete universal is in its intellectual aspect. If we assume that the deductive method is the only possible method of philosophy, we are driven logically to assume some great major premise or system of premises in order to justify and make possible knowledge—but should we find that deduction is not unitary in its nature, or should we find that there is another method of attaining knowledge than deduction, our monistic ideal of the concrete universal would become unnecessary.

While all knowledge is homogeneous inasmuch as it uses the rules of formal logic, it has that characteristic only because it is knowledge; we ought not to deny the possiblity of the existence of disparate systems, which agree only in that they are systems and use the rules of logic. While some systems are intrinsically homogeneous, as, for example, algebra and geometry, we may find others which are only partly homogeneous and partly non-homogeneous, such as the Euclidean and non-Euclidean geometries; and we may find others which are utterly disparate, such as the number system and a homogeneous fluid. The assumption that the systematic character of all knowledge shows that all knowledge or the universe is one system is an idol of the tribe. While monism may be true as a metaphysical theory, yet our theory of knowledge ought to be impartial as to monism and pluralism, and be itself capable of establishing either view, providing the appropriate evidence is presented. Too often theories of knowledge prejudice this issue.

§ 31. We must next ask the question, How do we establish and test the correctness of methods of deduction? No process of deduction can be accepted as having logical validity without critical examination and being compelled to prove its validity. The mere claim of any process of deduction to validity, such as the dialectical method of Hegel, cannot be accepted a priori. Logicians have mostly agreed that the dialectical development of concepts, in the form that Hegel stated it, does not represent any real process, and so they have rejected its claim to validity. This rejection shows us that even those philosophers who accept the claims of the a priori are nevertheless unwilling to accept the claims of any and everything to be a priori or valid. By what method can we test the claims of a system of deduction such as the Hegelian dialectic? No method of deduction can do so. According to the coherence theory in its non-absolutistic form, the Hegelian dialectic is valid, since it coheres to form a system. If we demand an absolutely all-embracing system by which to test truth, we must confess that we know none. Intuition has validity at most only for the individual who has it, and none for anyone else, unless it is taken on authority. The worth of authority needs to be examined by some other method than authority. Hence we are driven back to induction as the only test that can be made of such an ultimate problem as the truth of a method of deduction. The Hegelian dialectic is, moreover, condemned by an inductive test. Thinkers instinctively take it as a hypothesis to be verified; if it is true, it must be able to include all scientific and philosophical knowledge; but the progress of science since the time of Hegel has revealed many truths that either do not fit into the Hegelian scheme or contradict it; hence the Hegelian dialectic is condemned as failing to meet the inductive test.

In similar fashion, all other deductive processes must meet the test of induction. The appropriate method of deduction is always (in most cases just implicitly) a part of every hypothesis, and its correctness is established together with and by the proof of the hypothesis. Certain modes of deduction have been so often established thus that they are now taken for granted, and the necessity of their proof is not usually realized.

The correctness of a method of deduction is then tested at the same time as that of the hypothesis. The systematic method of deduction is part of the hypothesis, and should be specified in any attempt to state a hypothesis in complete form. When a hypothesis is to be developed by mathematical deduction, the fundamental principles of mathematical deduction form an integral part of the hypothesis. Sometimes a hypothesis that uses mathematical deduction does not include the whole of mathematics but only a part of it—irrational solutions are neglected in physics, and negative solutions are often likewise neglected; not because they are incorrectly deduced, but because that part of mathematics is not assumed in the hypothesis from which the deduction is made. In the mathematical deduction, irrational and negative quantities may be used, for it is more convenient to solve an equation in its general form than it would be to use all the special devices that would be necessary if the method of solution were restricted to using the particular sorts of quantities that are really under consideration; but when the solution is interpreted in physical terms, only such quantities are considered as were assumed in the original hypothesis. Just as the testing of the validity of each step of a deduction (the checking of a validating proposition) is by induction, so induction is the test of the truth of a deductive method as a whole.

§ 32. Let us now ask the question, Whence come such relationships as are exhibited in mathematical deduction or the Aristotelian syllogisms? Evidently they are derived by abstraction from experience. Experience furnishes us with complex wholes from which we abstract certain elements; logical implication is the coming together again of those elements that have been abstracted. Or, if the abstraction is carried to a sufficiently great degree, logical implication may be the coming together of the abstraction with the class of things from one of which it has been abstracted. I see an object, hear it called a chair, and discover that I can sit down upon it. Later I return to the same place, see the object, and say to myself, "If that is the chair, then I can sit upon it." The relationship of implication between the chair and its capacity for being sat upon is the expression of the resynthesis of two elements that have been abstracted from the original experience with the chair. If, in my original experience, I discover that the chair has legs, a bottom, and a hidden part underneath, then I can later assert the relation of implication to hold between the chair and the legs, etc. Not only so, I very soon discover that there are other objects similar to this chair, that these objects have similar qualities, and I then assert the relation of implication to hold between these other chairs and their capacity for being sat upon, their possession of legs, bottoms, etc. The truth of this relation of implication needs to be established by an inductive process. Implication is a mental device for reuniting the parts of a complex whole in a stream of thought, which, because of its discursive character, must deal, for the most part, with comparatively simple ideas. It is the concise expression of the relation that holds between two terms whenever one can be inductively or deductively established to occur, exist, or subsist, or to tend so to do, whenever the other occurs, exists, or subsists.

As such, implication is, in its origin, similar to the mythical process of "association."[1] Primitive man's logical processes do often seem to be no more than processes of association; when a ham-bone forms part of the same experience as the catching of a seal, the ham-bone comes to imply the catching of a seal, and we say that the Eskimo has committed the fallacy of *post hoc ergo propter hoc.* Man very soon finds that some elements of experience invariably come together and others do not; criticism separates some from others; consequently, implication comes to mean more than conjunction in experience. In dealing with physical things, implication comes to mean invariable conjunction. But as the causal concept is developed, we come to say that a cause implies its effect, even in cases in which the cause does not invariably produce the effect. Causal implication becomes more than invariable sequence, it becomes a *tendency* toward a sequence, which tendency is asserted to exist, even when the effect is not produced. The proof of the existence of such a tendency is only by the process of rational induction, in whatever manner it is discovered. Thus the relation of implication becomes more subtle and less directly derived from experience.

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[1] Association is called mythical because the term, association, implies that the associated elements are first perceived as distinct, and then united; in reality, they are first parts of one whole, and then separated.

In other fields, a set of relations are abstracted from one field of experience, and are found to be applicable to another field. Most of the relations used in (extensional) symbolic logic, and in much of mathematics, are derived from the properties of positions on a plane. The set of postulates for the Boole-Schroeder algebra of logic[1] can all be obtained in this manner. Once derived, they are found to be applicable to much more than the particular experience from which they were derived, to logical classes, and other similar experiences. Thus the relation of implication, which is produced by a resynthesis of abstractions from the complex experience of positions in a plane, is extended to other experiences, and comes to be used widely.

Since the relation of implication is thus derived from experience, we should expect that there would be as many different kinds of implication as there are different kinds of experience.

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[1] Lewis, *Survey of Symbolic Logic*, p. 119.

In part, such is the case. When we speak of the "logic of events," we are using more than a figure of speech; we have found in history a certain set of antecedents and consequents that are sufficiently regular and important to possess all the characteristics of a deductive chain; events do literally have their logic, just as much as positions on a plane. Likewise we speak of a logical move in chess, of the logic in character development, and of the inevitability and necessity in the development of the action in a drama. In music there is the logic of musical progression. There are some sorts of relations, however, which are found more widely than others—especially such sorts of relations as hold between positions on a plane or between definitions of genera and species; since such relations are found, not only in their original habitat, but in a great many other types of experience, and, since they are important for ordinary life, these relations have become the relations of deductive logic *par excellence*, so that the relations between other types of experiences are conceived of on the analogy of these relations. Hence the ordinary mathematical and Aristotelian logic has become *the* deductive logic. As long as we find ourselves able to adapt all forms of experience to the ordinary logic, we may be content to use it; but we should be ready, as soon as experience fails to fit into the forms of deduction in ordinary use, to try any other form of deduction that does fit the situation, provided it meets the inductive test. A study of the types of deduction used in some non-mathematical inductive arguments, such as the explanation of the connection between Apollo and the mouse,[1] or the arguments of historical critics for their conclusions,[2] show us that while it is probably true that, formally, all or most deductive argumentation can be fitted in-

[1] A. Lang, *Custom and Myth*, pp. 103 ff.; quoted in B. Bosanquet, *Logic*, Vol. II, chap. v, pp. 162 ff.

[2] A long and intricate example of this type of reasoning, with much non-mathematical and non-Aristotelian deduction, is to be found in the Introduction to *The Missal of St. Augustine's Abbey Canterbury*, by M. Rule.

to the existing rubrics, yet the material of such argument varies very widely.

Inasmuch as the usual modes of deduction are taken from one sphere of experience and applied to other spheres, we may expect that the laws of logic will take on different meanings in different spheres. This circumstance holds true of the law of non-contradiction, as of other logical laws. Where the use of any particular logical laws would lead to awkwardness, we preserve their indefeasibility by saying, "This law does not apply here." The eternal truth of such a mathematical law as $2+2=4$ is preserved by this means. That is, *if* we can analyze one part of our experience into 2 and another part into 2 more, and *if* the right sort of relations holds between them, then $2+2=4$. But if we cannot so analyze experience, or if the right sort of relations does not obtain, then "the laws of addition do not apply"—we rule out all negative instances by saying that this mathematical scheme does not apply to such. Two pots of water poured into two other pots of water do not make four—they overflow! Two rats and two cats when put together are much more likely to make only two! Two dollars and two dimes do not make four dollars or dimes, they make $2.20. The processes and laws of deduction are variously adapted and modified in different circumstances, according to the nature of the material under consideration. Thus any logical law, such as the law of non-contradiction, means different things under different circumstances; it is only the same in form; its indefeasibility is a legal fiction kept up by logical artifices.

§ 33. It is not necessary for us to consider at length the third step in the inductive process, that of checking with experience the consequences deduced from a hypothesis. In this step observation and experiment enter into the inductive process. An experiment is an endeavor to determine whether the appropriate phenomena appear under the conditions under which the hypothesis asserts that they appear, when these conditions can be artificially produced. Where artificial manipulation of conditions is impossible or undesirable, resort is had to mere observation. We have previously pointed out that experiment and observation are rarely at random, but are nearly always directed by some hypothesis. It is the deductions the observer or experimenter has made from the hypothesis that enable him to know what to watch in the continual flux of events and what changes to make in the environment. The importance of the hypothesis in the process of discovery has often been emphasized; it and its consequences are equally important in the step of verification. In the actual procedure of testing a scientific hypothesis this step of checking the consequences of the hypothesis with experience usually takes the larger part of the scientist's time; yet it need not occupy us longer, since we are only interested in the logical aspects of science. Consequently, it is merely necessary for us to note that, in order to be verified, a hypothesis must enable the deduction of consequences which check with experience, and that this process of checking is an essential part of the logical procedure of verification. The logic of this process of checking is that of the immediate description, which will be considered in the next chapter.[1] What degree of checking is necessary in order that a hypothesis may be considered to be verified is the problem which will occupy the next chapter.

APPENDIX

In order to illustrate the power of syllogistic analysis as applied to mathematical arguments, both to give logical rigor and to discover unperceived presuppositions, we give here a syllogistic analysis of the first demonstrated proposition in *Principia Mathematica.* Whitehead and Russell assert that they have made all their assumptions explicit[2] in their list of primitive propositions, and that they do not presuppose the operations of logic. We shall see that this demonstration, like all other demonstrations, falls into the form of syllogism or immediate inference; but that, owing to the authors' failure to understand the syllogistic nature of all deductive inference, they have failed in their announced attempt to exhibit all the primitive propositions upon which their system rests, in spite of their statement that "proofs will be given very fully."[1]

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[1] Cf. ch. vii. §4.

[2] *Principia,* Part I, sec. A, introduction to the section.

The first demonstrated proposition in the *Principia*, *2.01, is given as $\vdash : p \supset {\sim}p . \supset . {\sim}p$, which is the principle of the *reductio ad absurdum.* It is concisely demonstrated as follows:

$$\left[\text{Taut}\ \frac{\sim p}{p}\right] \quad \vdash : {\sim}p \vee {\sim}p . \supset . {\sim}p \qquad (1)$$

$$[(1).(*1{\cdot}01)] \quad \vdash : p \supset {\sim}p . \supset . {\sim}p$$

This proof is analyzed and stated in full syllogistic form below, with references to *all* those propositions which are mentioned by the authors.

FIRST SYLLOGISM

Maj. prem.: If p is a variable elementary proposition, ${\sim}p$ is a variable elementary proposition.[2]

Min. prem.: $\vdash . p$ is a variable elementary proposition.[3] (1)

Canon: The assertion of the antecedent of a hypothetical proposition permits the valid assertion of the consequent.[4]

Val. prop.: The canon of the hypothetical syllogism in the *modus ponens* is correctly applied to this syllogism.[5]

Concl.: $\vdash . {\sim}p$ is a variable elementary proposition. (2)

[1] *Ibid.*, p. 102.

[2] This is *1.7 of the *Principia*, but not quoted in the demonstration of *2.01. The term "variable" is added to it, in order to simplify the demonstration.

[3] This definition is implied in the introduction, but is *nowhere expressed.* It is not of the same sort as the "definitions" used in *Principia Mathematica.* That work considers as definitions only what we have called abbreviating definitions, such as, for example, the major premise in our sixth syllogism. This definition is a *statement* about p, classifying it in a certain group, and consequently needs to be asserted.

[4] A restatement of *Principia* *1.11.

[5] This validating proposition is proved true by inspection; each of the other validating propositions in the demonstration (no two are identical, though they may be similar) is likewise established.

SECOND SYLLOGISM

Maj. prem.: Any variable elementary proposition may be taken as a value of any other variable elementary proposition, in a proposition containing variable elementary propositions.[1]

Min. prem.: Same as (2).

Canon: Any value of a variable may be substituted for the variable, provided that the substitution is made for each case of the occurrence of the variable.[2] (3)

Val. prop.: The canon of the substitution of values is correctly applied to this syllogism.

Concl.: $\vdash . \sim p$ may be taken as a value of any other variable elementary proposition in a proposition containing variable elementary propositions. (4)

THIRD SYLLOGISM

Maj. prem.: Same as (4).

Min. prem.: Same as (1).

Canon: Same as (3).

Val. prop.: The canon of substitution of values is correctly applied to this syllogism.

Concl.: $\vdash . \sim p$ may be taken as a value of p in a proposition containing variable elementary propositions.[3] (5)

FOURTH SYLLOGISM

Maj. prem.: $\vdash : p \lor p . \supset . p$.[4]

Min. prem.: Same as (5).

Canon: Same as (3).

Val. prop.: The canon of the substitution of values is correctly applied to this syllogism.

Concl.: $\vdash : \sim p \lor \sim p . \supset . \sim p$. (6)

FIFTH SYLLOGISM

Maj. prem.: Same as (4).

Min. prem.: $\vdash . q$ is a variable elementary proposition.[5]

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[1] A primitive proposition, *not* stated in the *Principia.*

[2] This important canon of substitution of values is assumed and used throughout the *Principia*, but is *nowhere stated.*

[3] This proposition is expressed in the authors' demonstration by $\frac{\sim p}{p}$.

[4] *Principia* *1.2; in the authors' demonstration it is expressed by "Taut."

[5] A second definition which is presupposed but *not* expressed.

Canon: Same as (3).

Val. prop.: The canon of substitution of variables is correctly applied to this syllogism.

Concl.: $\vdash . \sim p$ may be taken as a value of q, in a proposition containing variable elementary propositions. (7)

SIXTH SYLLOGISM

Maj. prem.: $p \supset q . = . \sim p \vee q$ *Df.*[1]

Min. prem.: Same as (7).

Canon: Same as (3).

Val. prop.: The canon of substitution of values is correctly applied to this syllogism.

Concl.: $p \supset \sim p . = . \sim p \vee \sim p$ *Df*[2] (8)

SEVENTH SYLLOGISM

Maj. prem.: Same as (6).

Min. prem.: Same as (8).

Canon: A definiendum may be substituted for its definiens.[3]

Val. prop.: The canon of substitution of parts of a definition is applied correctly to this syllogism.

Concl.: $\vdash : p \supset \sim p . \supset . \sim p$ Q.E.D.

By this analysis we see that because of their failure to understand that every piece of deductive inference (except immediate inference) requires five propositions, the authors of *Principia Mathematica* have omitted the canon of combination in each of the syllogisms in the foregoing simple demonstration (except in the first syllogism). Failure to analyze the minor premises resulted in the omission of another primitive proposition (the major premise of the second syllogism), and of three definitions. We thus see that the list of primitive propositions in the *Principia* is far from complete.

It is not invariably the case in the demonstrations of the *Principia* that the canon is omitted; sometimes the authors see that it is necessary, as in the last step in the demonstration of *2.06, where they give [(1).(2).*1.11]; here (1) and (2) are the premises, and *1.11 is the canon.

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[1] *Principia* *1.01.

[2] The *Df* must be retained here to complete the meaning of the sign =, although this proposition is not itself a definition.

[3] Another important primitive proposition used throughout the *Principia*, but *nowhere expressed.*

The authors take the practical attitude of attempting to reach a demonstrated conclusion with the minimum of effort, but they sacrifice logical rigor in so doing. Such a practical attitude is quite justified in a *merely* mathematical work that aims to deduce mathematical propositions from their logical foundations, but the too exclusive engrossment by that attitude prevented the authors from stating completely the primitive propositions upon which their magnificent system rests, although they explicitly made an attempt to do so.[1] The only way in which an adequate enumeration of the primitive propositions of such a system as *Principia Mathematica* could be made and logical rigor attained would be to recognize the syllogistic character of each step of the reasoning, and to search for all the propositions implied in each step.

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[1] It should not be thought that we advocate putting all the demonstrations of the *Principia* into as cumbersome a form as this example. That form is merely to make clear the exact course of the argument. Mathematicians can easily devise means of stating all the steps in a brief compass. Provided they are understood, the validating propositions could be omitted.

CHAPTER VII

VERIFICATION

§ 1. If the logical process of rational induction, that is, of establishing the certainty of what claims to be knowledge, consists (with exceptions to be noted later) of the four steps: (1) the enunciation of a hypothesis, (2) the deduction of its consequences, (3) checking those consequences with experience, and (4) the decision as to whether the hypothesis is substantiated or not, then the question, when is a hypothesis verified, becomes of extreme importance. We shall now attempt to answer it, leaving the discussion of some particularly difficult problems, especially the problem whether certainty is possible, to subsequent chapters. The reader is begged to keep his mind open to the possibility that certainty may be obtained by induction, and to consider the conclusions of this and the next chapter as hypothetical—*if* certainty or truth can be obtained by inductive means, how can it be recognized?

The contrast between the ancient atomism, which failed entirely of proof for its brilliant hypothesis, and modern physics, which has measured and weighed not only the atom but also its constituent parts, shows us that we need not despair of verifying hypotheses. Ancient atomism lacked, in particular, both the powerful methods of mathematical induction, which have proved so indispensable to modern physics, and the army of investigators of many lands who now devote themselves to physical research. The ages subsequent to the discovery of the atomistic hypothesis despaired of its proof and so achieved none. The difference in possibility of verification which existed between modern and ancient atomism is doubtless likewise possible for philosophy, provided philosophers understand the method and conditions for verification, and do not despair, but seek for them as zealously as have the physical scientists. Such a hope ought to act for philosophers as did the "ideas of reason" for Kant, which pulled him on until he discovered in his moral philosophy a way of substantiating the very transcendent ideas that he had to reject in the first *Critique.* Without the pull and hope aroused by these "ideas," we should never have had his moral philosophy opening a new realm of thought. Similarly, without the hope and belief that verification *is* possible and truth *is* attainable, philosophy can never attain to the dignity of a science and deal with knowledge or anything more than speculative opinion.

In discussing this important problem, as in the discussion of other logical problems, it is important that we do not divorce ourselves from experience and try to think out a solution for ourselves, as Francis Bacon did. Logic has never advanced in this way. Logical progress has always been the result of systematizing and criticising the procedure of those thinkers who have been successful in solving problems. Hence we must adhere as closely as possible to the procedure of inductive thinkers, and found our conclusions upon their experience. Nevertheless, we have found that even the greatest of them was often not clear as to just what he was doing,[1] and so we must criticize their procedure, rather than accept it blindly. In this delicate task we can only do our best, and hope that later writers will correct our mistakes.

§ 2. In answer to the question, When is a hypothesis verified? there is, in general, only one answer, When it can be shown that this hypothesis and *only* this hypothesis can account for the facts. Such a general statement helps us very little; for it does not tell when a hypothesis may be said to be the only one that accounts for the facts. We must consider the process of verification more in detail.

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[1] Cf. chap. iii, §§ 10, 11.

§ 3. Before we do that, we must first disabuse our minds of a misconception. It has been widely believed, ever since the time of Bacon, that the process of establishing scientific truths consists in the *elimination* of rival hypotheses. Such a statement certainly does not represent the actual procedure of scientists. Sometimes the investigator takes one or two rival hypotheses into consideration, and shows their falsity. Often he quite neglects other hypotheses altogether. Never is there any attempt to seek out all possible hypotheses and disprove them. In Newton's *Principia*, which has been taken as the model of scientific proof by subsequent investigators, we find no other hypotheses taken into consideration besides his own. Newton propounds his fundamental postulates, deduces his propositions from them, and then shows that from his propositions can be deduced positions of the planets, etc., which correspond almost exactly with the observed positions. There is no use of the method of elimination in his historic proof of the law of gravitation. That fact alone should make us suspicious of the dictum that elimination is an essential part of induction. There was no other plausible hypothesis in the field, and so Newton had no need to consider other hypotheses. When the scientist does refute a few rival hypotheses, the logical significance of this procedure is very different from that of establishing one alternative by eliminating all others. Nowhere do we find investigators searching for *all* the possible variant hypotheses and laboriously disproving them. We shall see later that this procedure of scientists is abundantly justified from the abstract logical point of view, especially by the concept of the limited universal. Elimination has not been fundamental in the logical process of verifying hypotheses.

A little consideration of the nature of a disjunctive argument ought to have showed logicians how false is the view that induction is a purely negative process. The cogency of a disjunctive argument that proceeds by the negative process of eliminating all the alternatives except one depends entirely upon the previous establishment of the truth of the disjunctive major premise. This major premise can only be established by a positive process (unless it is reached in turn by elimination from another disjunctive major, in which case the positive process of proof is merely pushed one step farther back.) The negative process of disjunctive elimination presupposes a positive process of proof. This view that induction is eliminative is one of the two most mischievous misconceptions[1] in the contemporary understanding of the logic of induction. Elimination cannot be the method by which any hypothesis is established to be the only one that can account for the facts. A positive process is needed.

§ 4. If, then, the establishment of a hypothesis does not consist in the elimination of its rivals, how is a hypothesis verified? In answering this question, we must make a distinction between those hypotheses that are susceptible of direct verification, and those that are not so susceptible. In the case of a limited group of hypotheses, it is possible to hold in mind a hypothesis together with the experience which it describes, so that they can be compared directly. This sort of hypothesis may be called an *immediate description*. It occurs rarely, chiefly in accounts of an individual's experience. In the verification of this type of hypothesis deduction is not required, since the only question at issue is whether the hypothesis corresponds, point to point, with the experience that it describes. When I give, for example, an account of my perceptions or feelings and experiences during a thunderstorm, while the events are happening, my account is evidently not my feelings or experiences, but distinct from them. Yet my account, if I am scientifically minded, cannot be composed of any and every word or idea that enters my mind; it must be criticized and subjected to the scrutiny of logical processes before I can be satisfied with it. I choose and select my words, sometimes even alter an expression after I have uttered or written it, in order to get my account as exact as possible. Or if, after the storm is over, I give a description of my memory of the storm, I again carefully choose and select my expressions; upon recalling forgotten items of experience, I may even change or alter my account after first writing or telling it. Evidently there is a logical procedure of criticism in any serious attempt to describe an experience. That logical procedure consists in taking the (actual or proposed) description as a hypothesis,[1] and comparing it with the experience. Only when the description, as a hypothesis, does exactly fit the experience, is a seriously minded thinker satisfied. No deduction is necessary, because the hypothesis does not pretend to be other than what it is, a description of what is present to my mind. Both the experience and the hypothesis are co-present and so can be carefully compared. If the correspondence theory of truth is true anywhere, it is here. (We shall see later that the correspondence theory is only exactly true of this sort of knowledge.[2])

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[1] The other one is that induction consists merely in the establishment of what are herein called "empirical laws."

The extent of this sort of directly verifiable knowledge is far less than has been ordinarily supposed. If I try to do more than merely describe my own present experience, my hypothesis cannot be directly verified, for the things or events to which it refers cannot be co-present with it in the same experience. If, instead of describing my memory of an event, I try to describe the event itself from memory, my description becomes a hypothesis that cannot be directly verified, but can only be mediately substantiated. Or if, instead of describing my present experience, I describe what are called "objects" as distinct from my experience of those objects, my description again becomes a hypothesis that can only be mediately verified, for those "objects" are not themselves co-present with my description of them; only the experience of the "objects" is known. If for example, I describe the table as rectangular, instead of having the oblique and obtuse angles that rectangular tables almost always appear to have, I am going beyond my experience of the moment, and my hypothesis is no longer directly verifiable. Such descriptions may be called "perceptual descriptions"; they are not immediate descriptions in the foregoing sense of the term, but perceptual judgments, and are hypothetical constructs.[1] Again, if I try to describe how another person is feeling or what he is perceiving, my hypothesis is again incapable of direct verification. In other words, only those hypotheses that are descriptions of what is purely subjective are capable of direct verification and need no deduction.

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[1] That the term "hypothesis" is not strained when applied to an immediate description may be seen from the fact that the description must be present to the mind before it can be compared—the description is first made and then accepted, or else it is not criticized for correctness; the description is distinct from the experience and superimposed upon it. Thought, attention, and exact comparison are necessary to justify the acceptance of an immediate description.

[2] Cf. chap. xi, § 14.

From the foregoing considerations, it is apparent that hypotheses which are capable of direct verification are not often made, especially in ordinary life. The introspective psychologist may give such a subjective description, but correct introspection is notoriously difficult. The careful observer in the fields of astronomy, biology, anthropology, etc., records his own experiences as well as his interpretation of those experiences. In ordinary life, too, when challenged, we sometimes give a description of exactly what we see or feel, instead of describing the objects themselves; in reply to an objection we may say, "Well, anyway, that dark speck up there, which I thought was an aeroplane, seems to be shaped like a T." But such descriptions are very unusual. In ordinary life we are very little concerned with what other people see, but very much concerned with the objects themselves, and so, unless we are describing an emotion, we almost always describe the object and not our experience of the object. In science, the particular journalistic accounts of experience, the immediate descriptions, are relegated to footnotes or monographs or manuscript archives, and the bulk of scientific discussion concerns itself with inferences[1] from those "facts." A comparison of the space in astronomical discussion devoted to records of observations with that given to interpretations of those observations, will illustrate the usual estimate of the comparative importance of these directly verifiable hypotheses. For the most part, they are presupposed but not stated, and are only produced when the accuracy of a statement is pertinaciously challenged.

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[1] Cf. chap. xi, § 2.

In the case of emotions, the situation is different—immediate descriptions are commonly given, because there are no outer objects to which emotions are referred. The primitive idea that emotions, like objects, are external, long ago ceased to be popular, though many people still seem to hold it. Nevertheless, these descriptions of experience, these directly verifiable hypotheses, are the conceptual foundations upon which we rely when our knowledge is challenged.

The immediate description may be objected to as unscientific, since, from its very nature, it is private; it is essentially a description of one person's individual experience, and so its correctness may not seem capable of being tested by a second person. In psychology, the behaviorists make this one of their arguments against introspection. It must be admitted that if a person deliberately falsifies his account of his experience, it is difficult to say with certainty, "You did not have the experience you describe, but a different one"—yet exactly that can be done, providing I know enough about the situation. To me, his description of his experience becomes a hypothesis, to be tested by the method of rational induction; if the necessary deductions from that hypothesis do not correspond with what I know about the situation, I correctly judge him a liar. The difficulty is that we rarely know a situation with sufficient exactness to make such a test and be certain of our conclusion; the pertinent psychological laws of perception and emotional reaction, which are necessary in order to make an accurate deduction to another person's experience, are not usually well enough known. In simple cases, however, we do make such a test, and in this way discover the man who lies about his own inner experiences.

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[1] By inference is meant here not the deductive inference discussed in the previous chapter, but inductive inference—those hypotheses which can be established by the "facts" under consideration. "Inference" means two things—the deductive inference of the preceding chapter and the inductive inference here referred to. Inference is furthermore a process of thought or a procedure of stating reasoning in a form suitable for criticism.

Where there is no deliberate lying, there nevertheless may be incorrect reporting of experience, due to inadequate observation, prejudice, or lack of training in this difficult matter. Interpretations of experience may be included in a report of observations; mistakes may be made in observation; emotion may color the report. Is, then, immediate description of no value? If so, all observation is of no value! As a matter of experience, every science recognizes that certain individuals are good observers, and that the observations of some others are not reliable. It therefore rejects the immediate descriptions of some and accepts those of others. How is this distinction between observers made? By precisely the method that we use in distinguishing a lying from a true report of experience—by taking the report as a hypothesis, deducing from it the experience which ought to be obtained under certain conditions, and then comparing them with that experience. Usually this procedure takes the form of repeating the experiment or observation under as similar conditions as possible. The problem of deducing what the experience ought to be under different conditions is usually so difficult that we do not attempt it; instead we make the conditions similar. But absolute similarity is not necessary; deduction can usually discover what the experience ought to be under slightly different conditions. In this way a reliable report and a reliable reporter are distinguished from unreliable ones.

Is immediate description unreliable when it cannot be repeated? Certainly not. Sometimes circumstances are such that only one person can observe an important phenomenon. Does science therefore reject that piece of testimony because it is not corroborated? Sometimes it is rejected and sometimes not. Science recognizes that men are governed by habits—if a person is a good observer under one set of circumstances, he will also be a good observer under all similar sets of circumstances (although under different circumstances, as when a physicist attends a spiritualistic séance, he may not be a good observer). Then our problem is merely to check whether that individual's reports under that sort of circumstances are reliable; if they are, we can accept his immediate description, whether it has been checked or not. Every science recognizes the observations of some individuals to be so reliable that their observations will outweigh a great deal of contradictory evidence given by others. This fact is especially true when the contradictory evidence can be explained as due to (i.e., deduced from the supplementary hypothesis of) prejudice, emotion, or some other reason.

When, then, the reliability of the reporter can be determined apart from particular instances, there need be no hesitation in accepting an immediate description as knowledge, even though there should be no way of checking it directly. Immediate description must be accepted as a valid scientific method, in spite of the fact that it is sometimes misused, since such misuse can be discovered by the more comprehensive method of rational induction. In psychology, then, we must accept introspective records (after criticism)—not necessarily as accounts of what actually happened, but as accounts of what was experienced. Whether my account is veridical of the actual event or not, at least my introspection, if I am a good observer and unbiased, is veridical of my experience. That is all that immediate experience claims to be.

There is an easy reply to the objection that all immediate experience, all sense perception, since it is necessarily individual, is unscientific. Experience is the foundation on which all knowledge is based, and immediate description is the ultimate conceptual source of all knowledge. If you deny the validity of immediate description, you deny validity to any and every piece of knowledge, including your own statement! Such an ultimate skepticism can never be certain! You can never prove that there is no way in which immediate description can be validated. Otherwise you would be deriving the very criterion of the invalidity of all experience from experience itself. The fact that the sciences have accepted conceptualized accounts of sense experience to be evidential—not always of that which is obviously implied by that experience, but as evidential of the nature of sense experience itself—ought to be sufficient for the present, until we meet ultimate skepticisms in detailed discussion.[1]

While the immediate description is used chiefly in comparing conceptual statements with experience, to check the accuracy of a description, a process of direct comparison quite analogous to it is used in checking the validity of an analysis or a classification, in verifying the validating proposition of a syllogism, in checking the accuracy of a comparison, in subsuming a particular case under a general rule, and in other such circumstances. The analysis is taken as a hypothesis and held in mind together with the experience or meaning that is analyzed, then the two are directly compared. Or the material and formal propositions of the syllogism are held together in mind and the material is compared with the requirements stated by, or implied in, the formal propositions. The particular case and the general rule are simultaneously compared in the mind, and the one is observed to fit into the other. The logic of such a direct comparison is quite the same as that of the immediate description. The extreme simplicity of this process has led to its neglect by logicians—there is no way to conceptualize such a direct comparison of two meanings; consequently, the existence of such a verifying comparison has not been observed. Yet the wide use of analyses in philosophy shows the importance of such logical criticism, for analyses are not always self-evident or correct.

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[1] For a discussion of such skepticisms and a positive substantiation of the validity of experience, cf. chap. xi.

Perhaps the greatest difficulty in this process is to get a firm grasp of the experience that is being analyzed. Experiences are so flexible, our conception of our experience is so susceptible to change by suggestion, that incorrect analyses have sometimes been admitted to be correct, just because the incorrect analysis acts as a preconception to suggest to the critic a memory of the experience analyzed which conforms to the analysis itself! Making analyses is like introspection in that it takes training and a resolute endeavor to see exactly what is there in either case. Under such circumstances, perhaps the best procedure is to get someone else, who has different preconceptions, to check the analysis too, so that the effect of preconceptions is eliminated. A memory of an experience is often not to be trusted; comparison should be made with an experience when it is occurring rather than afterwards. The greatest difficulty lies in keeping one's mind open—toward which state of mind a keen realization of the difficulty and a resolution not to accept any unsubstantiated statement is perhaps the best help. Truth comes only to him who keeps ready to receive it by maintaining an attitude of resolute willingness to follow the truth wherever it may lead. The immediate description is a hypothesis that is directly verifiable by a process of comparison with experience, without any other logical process, and whose verification consequently requires exactness and care.

§ 5. Outside of this very limited range, hypotheses are not directly verifiable, and deduction is necessary. Our general statement that the logical procedure of induction is fourfold may be retained with the single exception of immediate description. In dealing with a statement about the reliability of a person's observations, in dealing with the content of someone else's experience, or in dealing with perceptual judgments, which pretend to describe objects, not experiences of objects, hypotheses are no longer directly verifiable, and so deduction is necessary. Deduction is the means whereby the bridge is made between the hypothesis, which refers to something outside my present experience (e.g., the content of another person's consciousness) and my own experience. Thereby I can have knowledge about what is outside of my experience (e.g., I can infer that another person is frightened—I make a hypothesis to that effect and substantiate it by my experience of seeing him run, my observation of the expression on his face, etc.). Hence the necessity of making deduction exact and apodictic—in order that it may be impossible to introduce any conclusion which is not directly implied by that hypothesis. Since the process of inductive proof consists in making a careful comparison between the deductions from a hypothesis and the experience that is obtainable under the conditions stated in those deductions, it is highly important to determine exactly what are the *necessary* deductions from a hypothesis. Otherwise mistaken acceptances or rejections of hypotheses are likely.

When may such an indirectly verifiable hypothesis be said to be the *only* one that fits the facts? We must face this question in its most difficult form—with reference to those larger scientific hypotheses that are most difficult of verification. Our general answer to this important question is that a hypothesis may be considered to be the only one that fits the facts when the deductions from that hypothesis have been carried out into a sufficiently large number of diverse fields or kinds of experience, and when the hypothesis has been shown to be able to explain[1] phenomena observed in those fields.

The most important feature in this criterion is the diversity of the fields (or the kinds of phenomena) in which the hypothesis is shown to explain phenomena. Where a hypothesis is able to account for the phenomena of only one field, it is possible that a number of other hypotheses might also account for those phenomena. Where it accounts for the phenomena of two fields, the number of possible hypotheses that fit the facts of experience is very much reduced. When the number of fields of experience is increased still further, a point is very soon reached where either only one hypothesis can explain the facts or only this hypothesis together with other hypotheses from which this hypothesis is deducible can account for the observed experiences.[2]

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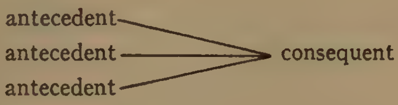
[1] By "explain" we mean that those phenomena are deducible from the hypothesis.

[2] The latter possibility, that a hypothesis may later be shown to be a special case of a still more inclusive hypothesis, will be dealt with in the next chapter, where it is shown that any hypothesis may be a limited universal.

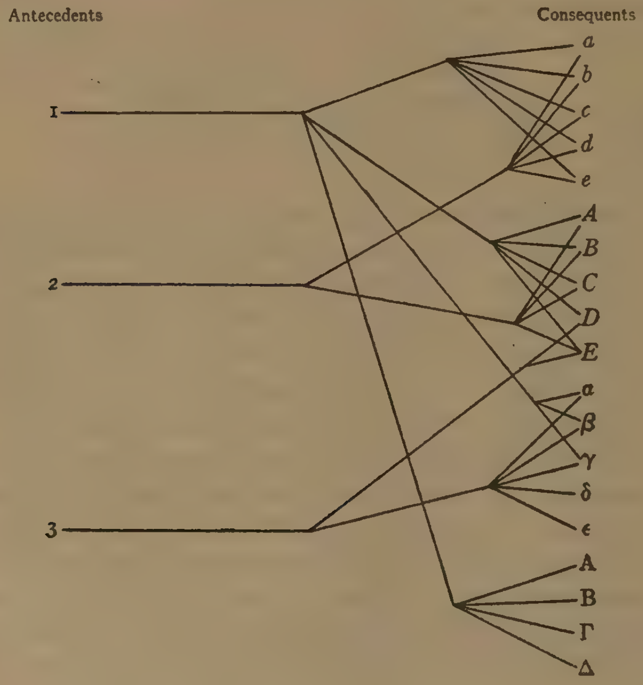
§ 6. The usual rejection of the possibility that certainty can be attained, because it is demanded that a hypothesis shall prove itself to be the *only* hypothesis that fits the case, is based on the logical conception of the hypothetical proposition and syllogism: that such a procedure requires the antecedent of a hypothetical proposition to be substantiated through the affirmation of its consequent—supposedly always a fallacious procedure.

Now the fallacious character of the affirmation of a consequent depends upon the conventional interpretation of the hypothetical proposition. This interpretation is that a hypothetical proposition expresses a condition in which an antecedent is invariably followed by a consequent, but the consequent may result from more than one antecedent. We may diagram the situation as follows:



As a result of this convention, when the consequent is asserted, no single antecedent may be asserted. For example, death may be the result of taking arsenic, strychnine, hanging, perforation of the heart, typhoid fever, etc.; while it is quite true to say that "If arsenic oxide is taken internally in sufficient quantity, death will follow," yet the occurrence of death does not justify us in asserting that arsenic poisoning has occurred.

We must admit that the foregoing statement is quite correct as an account of the meaning of a typical hypothetical proposition, but such a conception is an artificial simplification of the real situation. The actual condition, when all the possible and actual antecedents are known, is very much more complex. Each antecedent or set of antecedents gives rise not merely to one consequent but to a group of consequents, or several sets of groups of consequents. So complex are these systems of consequents, that, with possibly rare exceptions, if all the consequents of each set of antecedents were known, no two systems of consequents would be the same. Such has been found to be the case where systems of consequents have been studied extensively, as with the symptoms of diseases or the effects of poisons—when carefully enough observed, it has been found that the effects of the action of different kinds of disease germs always differ from each other, at least in some one way. Medicine has even discovered new varieties of diseases, such as the paratyphoids, by observation of slightly different sets of consequents. The true situation may be represented schematically as follows:



No antecedent is ever logically simple; nothing can be deduced from a single proposition; a set of propositions is needed. Arsenic alone produces no effects, it is arsenic introduced into the human body that produces death—a complex antecedent and a complex consequent. So we may regard 1, 2, and 3 in the foregoing diagram as complex sets of possible antecedents or complex hypotheses, from which follow sets of consequents or from which are deduced various consequents in different fields. If we consider only the consequents represented by the small Latin letters, the antecedents of hypotheses 1 and 2 could not be distinguished. If we extend our deduction and observation more widely, and include the capital Latin letters, we shall have a definite basis for distinguishing between 1 and 2, in that 2 fails to imply the consequent D, but instead implies E; yet if we take the supplementary hypothesis 3 along with 2, we shall have no basis for distinguishing 1 from 2 with 3. On the other hand, if we extend our deduction still farther to the field represented by the small Greek letters, we shall be able to distinguish clearly between the two sets of hypotheses, and shall also be able to reject the combination of 2 with 3, for the reason that consequents δ and ϵ follow from 2 and 3, but not from 1; furthermore, if we extend our deduction still farther, to the field represented by the capital Greek letters, we shall have unambiguous evidence for differentiating between the hypotheses, since hypothesis 1 alone enables us to infer these consequents.

That the true situation is the one represented by the diagram we have just been discussing, rather than that represented by the artificially simplified state of affairs implied in the ordinary hypothetical proposition, will be recognized by anyone who is familiar with a field in which the process of identifying an antecedent by the observation of consequents has been systematized, or in which the possible antecedents and consequents have been catalogued—such as the symptomology of diseases, the detection of poisons, or qualitative chemical analysis. The toxicologist knows that from the observation of death alone he is not able to infer anything as to its antecedent. All of the hundred-odd poisons he looks for have that in common. He also knows that symptoms of discomfort in the stomach, pain, nausea, vomiting, dizziness, headache, coldness, and convulsions are the consequents of more than one poison, or may be due to bacterial action as well as to poison. Such symptoms make him suspicious; nevertheless, if only these phenomena have been observed, he recognizes that no hypothesis can as yet be verified. If, however, after suitable treatment of parts of the cadaver, it is exposed to the action of nascent hydrogen, and the resultant gas is heated to ignition, producing a shining metallic mirror, he asserts that arsenic may have been the cause of death, although he is not sure, because bismuth also produces a mirror under these circumstances (a possible alternative hypothesis); but if the metallic mirror is dissolved by sodium hypochlorite solution, he pronounces positively (since bismuth is not so dissolved) that arsenic poisoning is the only antecedent which can account for the death and the other consequents that have been observed.[1]

In many cases there is some consequent that may be found to be "characteristic" of only one antecedent—in such cases the identification of the antecedent becomes easy (such a case is represented by the capital Greek letters in the foregoing diagram). In other cases, the situation is not so simple, and there may be no "characteristic" consequent; but it may be found that one antecedent produces certain sets of consequents, and that there are other antecedents which separately produce all of those consequents; but that each of these other antecedents fails to produce certain consequents, and that when two of them are combined, they produce in addition some other consequents which the first antecedent does not produce. In even such a complex case it is possible to identify an antecedent by its consequents. Such complexities and ambiguities are presented in the symptomology of diseases; but in each case, if the deduction from the hypothesis is carried out sufficiently widely, and if it is possible to carry observation to sufficient lengths, the antecedent is at length unambiguously determined; although physicians, being practical men who must act immediately, do not always wait for the completion of the process of induction before making their diagnoses and prescriptions. It is therefore possible in each case that, if a sufficiently wide investigation of possible consequents is made, the antecedent can be unambiguously determined. Although there are many problems as yet unsolved, so many apparently insolvable problems have been solved that there is no adequate reason for doubting that, given sufficient time, unambiguous consequents for most, if not all, actual antecedents will be found. We may therefore refuse to accept the solution of a problem as certain, until an unambiguous set of consequents is provided for the hypothesis. That such an unambiguous set can be provided is shown in the case of the toxicologist or analytic chemist. Moreover, it is rare that only one unique consequent or set of consequents can be found for a given antecedent; sufficient knowledge usually enables us to find several such. Consequently, *the deducing and verifying of a sufficiently wide and various range of consequents is sure to hit upon some unambiguous consequent or combination of consequents, and enable the certain establishment of their antecedent.*

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[1] The foregoing account is a description of the Marsh test for arsenic compounds.

If it is asked why, when we check with our experience a varied set of consequents drawn from a hypothesis, we should be enabled to select unambiguously an antecedent for those consequents; we reply that the reason is ultimately that, in a complex world, no two different rational antecedents, *when their consequents are fully drawn* in all possible spheres of action, have exactly the same consequents, so that the correct antecedent can be discovered by an examination of a sufficiently wide set of consequents. Where two antecedents have the same consequents, the reason is either because the antecedents are not really different or because their consequents are not drawn fully enough.

The rational inductive generalization is thus distinguished by the fact that from it can be drawn a wide range of deductions. The empirical generalization, by contrast, is one which consists merely in the statement of a uniformity of sequence, so that the only deduction which can be drawn from it is that a certain sequence can uniformly be found. It is hence incapable of a wide range of deductions, and consequently can never be certain (unless it can be deduced from a verified rational generalization), since it is incapable of deducing an unambiguous set of verified consequents.[1] We illustrate this important difference from the field of astronomy.

§ 7. The ancients found that the sun traveled in a path which they called the ecliptic, and that the sun and major planets all appear in a belt known as the zodiac. For them, these were merely observed uniformities or empirical generalizations. The first steps in the science of astronomy, as of other sciences, were empirical. It was not until the time of Kepler that the first rational law was established. The data upon which he worked were observations of the positions of the planet Mars, which were to him, from the viewpoint of logic, immediate descriptions. Kepler tried a number of hypotheses, and finally found that, by taking the hypothesis that Mars moves in an orbit which is an ellipse, he could deduce all the observed positions of the planet. Kepler's data had been gathered before the time of the invention of the telescope, and so were only approximate. He was favored in that he was dealing with a phenomenon whose gross behavior, we now know, is unusually simple; his uncritical acceptance of the belief that Nature always acts in the simplest manner aided him considerably, because it justified him in bringing his investigation to an end at this point, for there are other curves, of a higher order than the second, which might also have satisfied (enabled the correct deduction of) his data (i.e., Kepler failed to consider possible alternative hypotheses). While we recognize that Kepler by chance hit upon a law which is inherently simple (Kepler's law is exactly true when there are only two bodies in the system, i.e., no perturbations), we must remember that in the beginning of any science, only simple problems can be solved, and that the short steps of early geniuses were as difficult as the strides of later discoverers. Kepler's law gave an approximate statement of the behavior of the planet, which, because it was stated in quantitative terms, permitted a comparatively secure result. Whether Kepler achieved a verification of his laws is doubtful; an extension of his laws to the motion of other planets such as Mercury, the moon, etc., would have shown a host of discrepancies between the deductions from his laws and the observed positions of these planets (now known to be due to perturbing influences). Kepler fortunately realized neither the intricacy of the problem of determining the planetary motions nor the limitations of his own hypothesis, and so was saved the trouble of propounding supplementary hypotheses to account for the failure of his laws.

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[1] The phenomenon of the limited universal shows us that even a rational law is susceptible of degrees of wideness of application, and that the more general universal is shown to be such by its superior wideness of verified deductions.

Newton propounded and proved a broader generalization, which not only made Kepler's laws a deduction from his general law of gravitation but also enabled the deduction of deviations from Kepler's laws from the same hypothesis that Kepler's laws were deduced from. In addition there is deducible from Newton's hypothesis a wide range of phenomena, of which the most important are the following: (1) the succession of day and night; (2) the succession of the seasons; (3) eclipses of the sun and moon; (4) the phases of the moon; (5) the motions of the planets with respect to the fixed stars; (6) the precession of the equinoxes; (7) the motion of comets; (8) tides and trade winds; (9) the observed experience or fact that a weight dropped from a great height falls a little to the east of the vertical; (10) the fact that a pendulum set swinging in a north-and-south direction (in the northern temperate zone) appears to change its plane of vibration, and that a gyroscope appears to change its axis of rotation; (11) the fact that the equatorial diameter of the earth is greater than its polar diameter; (12) the fact that in the case of certain constellations, certain stars appear farther apart at certain dates than six months earlier or later. The first five of these facts of experience could be equally well explained by (i.e., be deducible from) the Ptolemaic hypothesis; but the others could not be so explained. The circumstance that the Newtonian hypothesis explains facts, not simply in the astronomical field, but also in other fields—the tides, many of the phenomena of meteorology, certain geographic characteristics of the earth, certain peculiar constituents of the bodies falling from a height, certain phenomena of pendulums and gyroscopes, etc.—shows that it is more than an empirical generalization; it is a rational hypothesis. The phenomena it explains are so diverse that it is the *only* hypothesis which could fit those facts. Nevertheless, it is possible that Newton's law is only a limited universal, and the Newtonian physics may be a special case of the more universal Einsteinian physics. This possibility, that every rational law may later be shown to be a limited universal, does not detract from the certainty of that rational law; Newton's laws have remained as true as ever, but their application may be shown to be limited in range.[1]

We may thus conclude that a hypothesis may be considered verified when it enables the drawing of a sufficiently wide range of verified deductions. It is interesting to find that Darwin appealed to the same criterion for his theory:

> This hypothesis may be tested . . . . by trying whether it explains several large and independent classes of facts; such as the geological succession of organic beings, their distribution in past and present times, and their mutual affinities and homologies. If the principle of natural selection does explain these and other large bodies of facts it ought to be received.[2]

§ 8. When is the range of verified deductions wide enough? In some cases (for example, Newton's theory of gravitation), that range is obviously so wide that no further discussion is needed. It is rare that so many different mathematically exact and verifiable deductions can be made as were possible from Newton's hypothesis; hence it often becomes necessary to determine more exactly what degree of wideness shall be considered sufficient. We can derive such a criterion from the fundamental logical demand that a verified hypothesis show itself to be the *only* hypothesis which can explain (permit the deduction of) the observed phenomena.

When a surpassing degree of wideness of verified deductions is not feasible, it is sufficient to show that a hypothesis (with the help of supplementary hypotheses, if necessary) furnishes a wider set of verified deductions than the nearest possible alternative hypothesis. It is not necessary to consider all possible alternative hypotheses; only those that seem likely to give the same or similar results need to be considered, since what is wanted is only a comparative standard of wideness of verification. Thus, while the wideness of the necessary range of verified deductions cannot be exactly specified, it can be specified sufficiently for logical purposes, since safety can always be secured by requiring too much rather than too little. A good hypothesis ought to be able to fulfil more than barely the minimum requirements for verification, and science is fortunately not in a hurry. She can wait a generation or more, if necessary.

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[1] Cf. chap. viii, § 1.

[2] Quoted in J. E. Creighton, *An Introductory Logic*, p. 291.

The ideal case is that in which the hypothesis under consideration permits the deduction of an additional phenomenon which cannot be deduced from any rival hypothesis, and in which this additional phenomenon is found. It is by restricting attention to this "crucial instance" that the opinion has arisen that valid inference may be attained by only one experiment.[1] In reality, the validity of such a hypothesis is established by a host of other verified deductions plus this one instance on which attention is concentrated because of its crucial character.

An analysis of the argument of such a scientific treatise as Charles Darwin's *Formation of Vegetable Mould through the Action of Worms* will illustrate the use of the foregoing criterion. One of Darwin's theses is that superficial objects tend to sink below the surface of the ground, because of the large quantity of fine earth continually brought to the surface in the form of castings by earthworms. In support of this contention he not only adduces positive evidence but he also shows that the nearest alternative theories fail to deduce the facts. In reply to the suggestion that worms are too weak to produce such great effects, he secures elaborate statistics of the amount of earth brought to the surface in the form of castings and shows that the amount corresponds with the amount his theory requires (a verification of a deduction from his theory). The common explanation (the alternative hypothesis) had been that heavy objects sink in soft soil through their own weight; Darwin shows that not only heavy objects are found beneath the surface of the soil, but sand and light objects also sink, and that when chalk had been spread on the surface of a field, it was found, years later, as a horizontal stratum some inches beneath the surface, with its lighter and heavier particles intermixed. Thus Darwin not only shows that his theory is able to permit the deduction of a wider range of phenomena than the previously accepted theory, but he produces a crucial exception: he shows that the observed phenomena contradict deductions from the accepted theory—that theory requires that objects should sink in proportion to their weight, whereas light and heavy objects sank almost equally. Darwin's treatment of alternative hypotheses is logically excellent—he compares the facts of experience with two hypotheses: his own and the nearest alternative hypothesis.

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[1] Hume, *Treatise*, Bk. I, Pt. III, sec. viii, p. 104.

It may be objected that our knowledge of alternative hypotheses may be limited; that it is possible that there may be an alternative hypothesis which is closer than any one that we are considering; hence our criterion that a hypothesis should provide for a wider range of verified deductions than the nearest alternative hypothesis may fail for want of knowledge of the relevant alternative hypothesis. Here there are two cases: (1) where this unknown alternative hypothesis is fundamentally similar to the first one, and (2) where the known and unknown hypotheses are not fundamentally similar.

If our two hypotheses, known and unknown, are fundamentally similar, it is not necessary to compare them, for the relation between them, if one is shown to be true, is that of more and less limited universals. Thus (assuming the still unsettled truth of the theory of relativity) it would have been quite unnecessary for Newton to have considered the relativity theory as an alternative hypothesis to his own, since the two are fundamentally the same—from Einstein's hypothesis can be deduced all the effects that Newton knew; the difference is that the theory of relativity enables the deduction of certain effects which Newton did not know and which cannot be deduced from his hypothesis. The truth of Einsteinian physics would not disprove that of the Newtonian physics, for Newtonian physics is a special case of Einsteinian physics. Hence, it is unnecessary to search for generalizations which are fundamentally similar to the one that we are testing, for such generalizations are not properly alternative; we may confine ourselves to those generalizations which are fundamentally different, since a limited universal is not discarded when a wider generalization is established (just as Kepler's laws were retained by Newton); its limits are merely pointed out. This consideration limits very considerably the labor necessary to establish the correctness of a generalization that does not provide a superlatively wide range of verified deductions.

As to the second case, it must be admitted that, given a hypothesis, it is difficult to discover all possible alternative hypotheses. Yet the human mind is very flexible; give it a problem, and, in the course of time, a large number of diverse solutions will be proposed, unless one should happen to satisfy men's minds. If there is a body of active thinkers interested in a problem, mutually criticizing one another, then sooner or later all relevant solutions of the problem will be proposed. It must be remembered that while detailed solutions of a problem may be many, the number of *kinds* of solutions is very much less, and the number of general sorts of kinds of solutions is comparatively few. Consequently, it is by no means impossible for one individual, if his mind is unusually alert, or for a group of individuals, provided they are open-minded, to think of all possible sorts of solutions to any problem.

An illustration of this fact from a field where such phenomena are not usually expected is to be found in the criticism of Descartes' philosophy that he published with his views. He invited criticisms from the outstanding thinkers of his time, and it is a remarkable fact that practically all the criticisms of Descartes' philosophy which appeared in the subsequent and unusually fruitful century of philosophic thought were explicitly or implicitly mentioned in that series of criticisms. Had men been more open-minded and less inclined to follow the authority of a great name, philosophy would have been spared the necessity of laboriously removing the prejudices with which Descartes imbued the philosophic world. Open-mindedness, a group of active critical minds considering a problem, and time, are alone necessary to suggest all the possible alternative solutions.

In the solution of problems that are not intrinsically difficult, the factor of time becomes unimportant, for it is not necessary to wait long before the important alternatives can be found. Where prejudices are many or strong, time becomes very important, since it may take generations for these prejudices to be overcome—it was mainly prejudice that prevented the ancients from solving the problem of the configuration of the solar system. Where there are only a few individuals considering a problem, time may again become important. But where there is an active and numerous group of critical minds engaged on a problem, competing with each other for the honor of discovering and demonstrating the correct solution, the length of time a solution has been before the scientific world to insure all relevant alternative hypotheses being discovered need not be great, unless the problem is extremely complex. The present condition of the natural sciences, when different nations are struggling with the same problems; with scientific journals conveying information, suggestions, and criticism from one group to another; with an intense competition between scientists, and great rewards in the shape of scientific fame for the successful discoverer or critic—such a condition is especially conducive to the rapid appearance of all relevant solutions to a problem, with the result that it becomes comparatively easy to tell when a hypothesis has produced a sufficiently wide range of verified deductions to establish itself as true. In more complex and difficult problems, it may be necessary to suspend judgment and wait; science, being a theoretical pursuit, does not need to solve its problems upon the instant, and can wait as long as necessary. The greatest difficulty, of course, is to discover an unambiguous deducible consequent that is susceptible of verification; hence the final decision upon the truth of a hypothesis may have to be held in abeyance for a very long period, not because of any logical necessity, but because no decisive verifiable deduction is available. Thus Dalton's hypothesis of the existence of atoms had to wait a century before being verified by conclusive experiment. The length of time a hypothesis has been before the scientific world is not usually a factor of importance. The degree of wideness of verified deductions is decisive, and, if a superlatively wide range of verified deductions is obtainable, it is not necessary to consider alternative hypotheses; but, where this range is not unusually large, the requisite degree is determined by comparison with relevant alternative hypotheses.

§ 9. Logic does not simply state what is the correct procedure in reasoning, it also warns against failure to obey its rules, by enumerating the fallacies that transgressors against the laws of logic fall into. The assertion that a hypothesis is established without a sufficiently wide range of verified deductions constitutes the fallacy of *hasty generalization*. Usually this fallacy is committed when a person finds that a single deduction from his hypothesis is verified, and immediately concludes that the hypothesis is correct. But the fallacy need not be due to inference from merely a single verified deduction; it may be committed by concluding that a proposition is true with any number less than a sufficient number of verified deductions. The larger, however, the number of verified deductions, the less heinous becomes the offense; it must be remembered that *faith* in an unproved hypothesis is the driving force behind successful verification. The fallacy of hasty generalization is committed, not when there is faith in the generalization coupled with an attempt to verify it completely, recognizing that verification is as yet incomplete, but when belief is dogmatic, and no further verification is attempted or thought necessary. This fallacy is usually caused by failure to keep the mind open to further evidence.

§ 10. With this understanding of the nature of inductive proof, we can see wherein lies the advantage of mathematical or quantitative measurement. Since the verification of a hypothesis ultimately consists in showing that one hypothesis alone enables us to deduce the observed phenomena, it is an advantage to have the phenomena as unique as possible. If the deduction from a hypothesis is that light will be produced, light might be caused in very many different ways (the number of possible antecedents is large); but if the deduction is that light of one particular wave-length alone will be found, and that wave-length can be stated in quantitative terms, the number of possible causes is restricted to a very few. Quantitative determination of a consequent restricts the field of possible antecedents in proportion to the degree of accuracy of measurement.

Consequently, one accurate quantitatively verifiable deduction is as good as a great many merely qualitative deductions, unless the qualitative phenomenon is peculiarly unique. (This superiority is gauged by comparing alternative hypotheses that deduce the phenomena under consideration.) The superior power of determining their antecedents, which quantitative results have, has been responsible for the greater progress of those sciences that can use quantitative methods. Thus the difference between quantitative methods and qualitative methods is logically one of degree, not one of kind. It is just as possible to verify a hypothesis that is only susceptible of qualitative deduction as it is to verify one that is susceptible of quantitative deduction. Both sorts of deduction are logically of the same nature; the difference is that a larger range of qualitative deductions may be necessary to insure the truth of a hypothesis than would be necessary were the deductions statable in quantitative form. Mathematical physics does not "transform everything qualitative into quantitative equivalents";[1] it makes its hypotheses susceptible of mathematical deduction; but the results of that deduction, when interpreted in physical terms, are just as qualitative as the hypothesis is. It is literally impossible to make purely qualitative phenomena into quantitative ones, as the biologist has found to his cost. Indeed, if quality is or can be made *equivalent* to quantity, then that quality is already quantitative!

It is likewise erroneous to say that qualitative statements are not as accurate as quantitative statements; purely qualitative statements may be *more* accurate than quantitative statements, for qualities are often unique, whereas quantities can rarely be determined except by approximations. Such statements as "The blood of animals circulates through the body" or "My dog is dead" are more exactly accurate than such a statement as "The meter is 39.37 inches long" or "The wave-length of this light is 390 $\mu\mu$." The superiority of the quantitative sciences is thus merely one of degree, and qualitative sciences are able to establish their conclusions with an equal degree of certainty, though with greater difficulty.

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[1] J. Ward, *Naturalism and Agnosticism*, I, 98.

§ 11. While the chief reliance for the proof of a hypothesis must be placed upon a set of verifiable deductions in diverse fields, there are certain supplementary criteria that furnish valuable assistance.

Jevons' insistence that a hypothesis must allow of the application of deductive reasoning, and that it must permit the exact deduction of consequences which are capable of comparison with the results of observation, is very much to the point (except in the case of an immediate description). A hypothesis from which nothing can be deduced can never be verified. Not only must deduction be possible, but the hypothesis must be stated in such a fashion that it is possible to make the deduction sufficiently exact so that if the hypothesis fails to explain the phenomena, that failure will be evident. It is in this respect that mathematics is so valuable. The ancients often erred in their failure to make exact deductions, and it was the loose use of such ambiguous terms as "strange," "common," "natural," and "unnatural," that made their science so sterile. With such loose terms it was impossible for them to tell when they were wrong, so that no advance was possible. Scientific definition is a prerequisite for successful verification.

The hypothesis that a phenomenon is due to the action of spirits suffers from this sort of difficulty. The appeal to spiritual agencies has always been objected to by scientific men; the difficulty is that such an appeal is utterly ambiguous. Spirits are such ill-determined things that nothing in particular can be deduced from a hypothesis which appeals to their agency. If I come into a room, find the chairs in disarray, and make the hypothesis that there has been a football rally, or a round-table discussion, or an examination, or a free-for-all fight, in each case, a particular sort of arrangement of the chairs can be deduced from my hypothesis, and verification may be possible. But if I say that the disorder was due to the action of spirits, nothing specific can be deduced. If spirits had some specific manner of action, scientific investigators would not object to the use of hypotheses involving them; the reason that science rejects the appeal to spirits is that this hypothesis is utterly ambiguous.

Metaphysics has also tended to err in this direction; while speculative systems logically have been hypotheses, yet too often the system has been stated in such vague terms that it is impossible to pin the metaphysician down to an exact statement of the implications of his theory. Such terms as "reality," "absolute," "knowledge," are used in such a loose manner that, for example, it is impossible to tell whether a particular statement is "knowledge" or not. Under these circumstances, a metaphysical theory is irrefutable, because it can adapt itself to any and every situation; but it is likewise unprovable.

It is also important that the consequences which follow from a hypothesis shall be such that they can be compared exactly with human experiences. The ancient atomists were in the position of having propounded a theory that allowed of only a limited set of deductions; they could deduce from their theory the hardness or softness of things, their figure, motion, etc., but they were unable to deduce any specific degrees of these qualities, which could be confirmed or rejected by exact investigation. Other deductions were possible, such as that, with keen enough vision, atoms should be perceived, or that, if the atoms were removed, nothing would be left, etc.; but such deductions could not be confirmed. Modern atomism, because of the superior exactness with which its hypotheses are framed, and the vastly greater power of its mathematical deduction, is able to deduce consequences from its hypotheses which are capable of observation and of exact measurement, and has established itself as knowledge, where ancient atomism failed.

Metaphysics has traditionally dealt with hypotheses that are unverifiable because they deal with matters that are beyond experience. The implication is that a metaphysical hypothesis has no consequences which can be uniquely found in human experience, and hence cannot be verified.[1] If any philosophical speculation is so vast and so removed from human life that no consequence can be deduced from it which can be checked with human experience or which has effects upon human life, then that speculation is totally impossible of verification or rejection. It is a speculation, pure and simple, and has no place in science (taking "science" in the sense of any discipline that treats of knowledge or what may become knowledge). In part, philosophy has consisted of just such speculations.[2]

Nevertheless, human beings, even philosophers, spend little time on such fruitless speculations. Metaphysics has concerned itself more with a different sort of hypotheses—those whose inductive verification is impossible, because unambiguous consequents are lacking in human experience, but whose acceptance or rejection makes a great difference to human life because of the effect that *belief* in them makes upon human life. Even though we may not be able to decide scientifically between the hypotheses of optimism or pessimism, theism or atheism, monism or pluralism, yet the acceptance of one or the other of those alternatives may make a very great difference in our lives. In such cases, philosophy has ventured into the sphere of religion, for it is religion that concerns itself with whatever is most important in the universe and most fateful for man, especially when that object or objects is impossible of scientific demonstration. Most of the classic philosophies (such as Spinoza's) have dealt chiefly with this sort of problem; they have consequently been philosophical (i.e., reasoned out) religions. The field of philosophy has been difficult to define because its subject matter has been so various; the only common characteristic its subject matter has had was the one common to all intellectual pursuits, that it was (partly) reasoned out. Philosophy has thus been partly science,[1] partly speculation, and partly religion.

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[1] The discussion as to whether a hypothesis that concerns itself with what is beyond experience (i.e., one that transcends experience) can be verified, must be left to chap. xiv.

[2] There are other speculations that belong to science. Such are those suggested (but as yet unverified) hypotheses that later may very likely be verified or rejected. These hypotheses are to be distinguished from other speculations in that they have some consequences that are to be found in experience, but not an adequate number of them. Science needs a supply of notions toward which to work—its working hypotheses or speculations. Formerly, philosophy furnished many such; today they, together with most knowledge, belong to the special sciences.

William James's appeal to the will to believe is unjustified as a procedure of science, because the situation in which he conceived the exercise of the will alone to be justified cannot produce knowledge—when an insolvable problem is sufficiently important for practical considerations to compel the making of a decision. James's appeal is to faith—which is a matter of opinion, not knowledge—and is justified in its own field, that of practical life or religion; not in the field of science.

The failure of philosophy to solve its problems scientifically has not always been due to the fact that it has been dealing with insolvable problems, but largely to the fact that its hypotheses have been too vague to permit of either verification or refutation, or else that philosophers have not sought out and discovered sufficiently exact methods of deduction to permit the testing of the consequences of a hypothesis. In part the difficulty has also been due to the fact that philosophers have not realized what is the only correct method of attaining knowledge—the method of rational induction. In view of the difference in logical situation between ancient and modern atomism, does it not behoove philosophers to purge their hypotheses of ambiguity and loose thinking, and to search for methods of making accurate and fruitful deductions from these hypotheses, instead of idly saying that philosophic theories are beyond verification?

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[1] In so far as its conclusions are susceptible of rational inductive demonstration—to be found chiefly in the fields of logic, epistemology, aesthetics, and ethics.

§ 12. A second subsidiary criterion for the truth of a hypothesis is that we can only admit so much of a hypothesis to be verified as is necessary in order to deduce those consequences upon which the truth or falsity of our hypothesis hangs. Suppose we had a complex hypothesis, and that from one part of it no verifiable consequences could be deduced, whereas, from another part, important and verifiable consequences could be derived; we could not conclude upon verifying the deducible part of the hypothesis that it is true as a whole, but we should be compelled to say that only so much of the hypothesis is true as is necessary to deduce the verified consequences, and *no more*.

This is as far as we can go in accepting the "principle of parsimony";[1] it is the element of truth in that principle which has made it persuasive and appear to be constantly used in science. We shall call it the principle of the non-verification of otiose elements in a hypothesis. If we should include in the Copernical hypothesis the additional element that "beyond the orbit of the planet Neptune, the last of the planets, at a distance many millions of miles greater than that of Neptune from the sun, there is a silver teapot which revolves regularly about the sun just as the planets do, although with a much longer 'year,' "[2] we should not be able to use that part of our complex hypothesis which refers to the silver teapot in any significant deduction; so we should have to say that only the remainder of our Copernican hypothesis is verified. In this interpretation

[1] Cf. chap. viii, § 2.

[2] Quoted in R. H. Dotterer, *Beginners' Logic*, p. 314.

of the "law of parsimony"* or the "principle of simplicity," we are not assuming, as the early modern scientists did, that nature works in the simplest way, but merely stating a consequence of the hypothetical method of induction. Otiose elements of a hypothesis are not established in the verification of that hypothesis.

§ 13. It also follows from our conception of the way in which a hypothesis is shown to be the only one which fits the facts that the "principle of superimposition" is a valuable criterion. When we find a hypothesis that explains (i.e., from which are deducible) all the facts that a formerly accepted hypothesis explains, and other facts in addition, we may conclude that our new hypothesis is pretty sure to be a more general truth in a field in which our formerly accepted hypothesis is a limited universal. Since the proof of a hypothesis consists in finding and verifying deductions in as many fields as possible, the larger the number of fields covered by a hypothesis, the more likely it is to be true. On the other hand, when hypotheses merely overlap, so that hypothesis *A* explains a set of facts and hypothesis *B* explains only some of the facts that *A* explains, but not all, even though it explains other facts in addition, there is no superimposition, merely two rival hypotheses between which we cannot as yet decide. The existence of negative instances to both hypotheses would prevent us from considering either one as true. On the other hand, a hypothesis which is merely wider in its scope cannot be accepted on that account alone, for our previous principle of accepting no more than is necessary to deduce the verifiable facts prevents us from taking mere inclusiveness or wideness of generalization as a worth-while consideration. The additional facts that are deducible from the wider hypothesis must be found in experience.

Examples showing that the principle of superimposition is useful are many; indeed, wherever one view has supplanted another, it will be found that the new view has included all the facts which the old one included, and more besides. This phenomenon is so pervasive that Whewell made it one of the most important of his criteria of truth, and prepared tables illustrating the way in which one conception was superimposed upon another.[1] The relations of the Copernican to the Ptolemaic theory, of heat as molecular motion to heat as phlogiston, of Einsteinian relativity to Newtonian physics, are all of them relations of superimposition. Hegel turned this logical characteristic of truth into his dialectical process of thought. The superimposition of new truth upon old, which makes every verified hypothesis a possibly limited universal, is the truth that gives plausibility to the coherence theory. Coherence is a valuable criterion, but is not in itself an adequate one. The only criterion of truth that we can unreservedly accept is the verification of a wide series of deductions from a hypothesis.

§ 14. We must reject the criterion of conceivability as of no value in induction. Conceivability is so much a subjective matter, so relative to the state of knowledge of the subject and to the vividness or liveliness of imagination of the individual, that we must refuse to it any probative force. No hypothesis that can be stated in words is literally inconceivable; if it were inconceivable, it could not have been stated! Even such self-contradictory notions as "round squares" are conceivable in conceptual terms and it is possible to make statements about them, at least the statement that they do not exist! If we are not prepared to accept the concept of "round squares" as conceivable, we ought also to reject the concept of the null class as inconceivable, for it possesses all the characteristics of round squares; and yet the concept of the null class has filled a useful place in the recent developments of logic. If, on the other hand, conceivability is interpreted as the quality of being imageable, we must reject all concepts or abstract ideas as being inconceivable.

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[1] Cf. the illuminating tables in *Novum Organum Renovatum*, p. 140.

In actual use, the criterion of conceivability has usually meant the characteristic of being homogeneous with received notions; with that meaning, it is a very useful criterion, for it serves to reject many idle vagaries of thought; but it has no logical force, for most new and great conceptions have gone contrary to received notions. When we remember that Newton's gravitational hypothesis was objected to on the ground that it implies action at a distance, which was inconceivable, we see how futile is the criterion of conceivability and inconceivability. It is thus neither the case that every conceivable hypothesis is true nor is it certain that every inconceivable hypothesis is false.

On the other hand, logical absurdity or inconceivability becomes a valuable notion when it comes to mean "logical inconsistency with correct deduction from a known truth." If we have a true hypothesis (we must remember that a method of deduction is part of every verifiable hypothesis), and if we know that our hypothesis is not a limited universal in any particular (restricted) field, then we can conclude that any proposition which is inconsistent with a deduction from that hypothesis is untrue in that field. Wherever Euclidean geometry is true, "The sum of the angles of a triangle is equal to more than two right angles" is an absurd (or false) proposition. But if elliptic geometry is true of a particular sphere of experience, that proposition would be true. Logical absurdity is thus seen to be an important concept, but one that is confined to deductive use, for it presupposes the knowledge of a truth from which is deduced the proposition to which our given proposition is logically repugnant. The notion of logical absurdity (or inconceivability) must thus be limited in its proper use to deduction, and it can be of no use as an inductive criterion.

§ 15. The most striking of these subsidiary criteria for the correctness of a hypothesis is that of prediction. When a hypothesis is able to predict some phenomenon not previously known and when that phenomenon is discovered subsequently to its prediction, we naturally feel that such a hypothesis must be true. Newton and other astronomers made prediction of previously unobserved phenomena an important feature in the verification of their hypotheses. The reason for the value of prediction is that where a previously unknown event is predicted, the hypothesis cannot (in this respect at least) be *ad hoc*.[1] Since a hypothesis is usually formed as the result of observation and generalization of phenomena, it must, to a certain extent at least, be *ad hoc*, in that it explains the phenomena from whose generalization it arose. The difficulty of the *ad hoc* hypothesis is due to the fact that such a hypothesis is unable without a change to extend itself to fit new facts other than those upon which it was originally based—but a change in a hypothesis is logically the destruction of the first hypothesis and the formation of another! The *ad hoc* hypothesis is thus one that is unable to cover a sufficiently wide range of phenomena. It is therefore a most valuable confirmation of the truth of a hypothesis that it is able to extend its deductions into entirely new fields, or to previously unobserved phenomena in old fields. We are then quite justified in making prediction an important criterion for the truth of a hypothesis. Because of its psychological impressiveness, we ought not, however, to overestimate its value; we should remember that the criterion of prediction must always be used in conjunction with the general criterion of wideness of verifiable deductions, since it is quite possible that a false hypothesis might accidentally predict an unknown event, as professional prophets sometimes hit upon the truth.

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[1] An exception occurs where the hypothesis is a mere empirical generalization, such as the prediction of the tides at any place by taking the average tide for that day for a period of years. A successful prediction of this sort is usually *ad hoc*. In this case the event predicted is of the same sort as those on which the generalization is based. Cf. § 16.

Nevertheless, the criterion of prediction is so important that in our present state of imperfect knowledge we may well make it a requirement of every significant hypothesis which pretends to be new, and which is a candidate for truth, that it should predict successfully some previously unknown phenomenon. At present our knowledge seems to be a drop in an ocean of ignorance, and so we may even reject a hypothesis (unless it is one that does not pretend to be new or significant) if it cannot successfully predict anything. Prediction has been such a prominent and omnipresent phenomenon in the recent development of science that it is hardly necessary to give examples. We may mention the way in which Mendeléef's table, although itself only an empirical generalization, was yet able to predict the existence and properties of previously unknown chemical elements. One of the strongest evidences for the truth of the Bohr theory of the atom is the way in which it is able to explain the chemical properties of the various elements, so that it could predict the properties and probable location of a previously undiscovered element, hafnium. The corroboration of the general theory of relativity depends largely upon whether Einstein has been able to predict successfully two new phenomena, the aberration of the light of those stars which passes near the sun and the displacement of lines in the spectrum of light coming from a strong gravitational field; his successful prediction is urged as one of the most important reasons for accepting that theory.[1]

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[1] Physicists are searching for an alternative hypothesis that may enable the deduction of similar consequences. If one should be found, the further question would arise: Are there any verifiable consequences that follow from one hypothesis and not from the other? If so, one hypothesis can be eliminated; if not, we shall be left with two unverified theories. Cf. ch. xii, § 6, note 2.

On the other hand, when human knowledge becomes greater, so that human ignorance is not so abysmal as at present, prediction will become much less important than it is now. In certain fields, where the phenomena have been long observed and are well known, prediction is even now less important. Such seems to be the case in the field of philosophy. The study of philosophy has attracted the best minds for such a long period, and the phenomena of the inner life have been so much observed, that it is quite possible that no important discovery of *facts* is to be expected in that sphere, and that even new hypotheses are hardly to be looked for, but merely the reworking and making more exact and deducible of previously enunciated hypotheses. Under these circumstances, reliance must be had upon the wideness of verifiable consequences, rather than upon prediction. While prediction is the most striking characteristic of a rationally established law, yet it cannot be invariably required.

§ 16. It has been said that a single exception will upset an inductive generalization. We shall see later that this statement is only true (in most cases) of the empirical generalization, but not of the verified rational inductive generalization. In some cases, where a generalization has never been completely verified, the discovery of an exception may overthrow, wholly or partly, that generalization. Such has been the case with the undulatory theory of light, which has never quite succeeded in overthrowing its rival, the corpuscular theory, and which at last seems itself to be overthrown by quantum phenomena. But where a rational inductive generalization has been established, the discovery of an exception does not overthrow the law; it merely indicates either that a supplementary hypothesis is needed or that the law is a limited universal. The secular motion of the perihelion of the orbit of Mercury has always been an apparent exception to Newton's laws, yet Newton's laws were so very widely verified that the existence of this apparent exception did not prevent the universal acceptance of those laws. It was supposed that some supplementary hypothesis, such as an intro-Mercurial planet, or the matter of the zodiacal light, would explain (i.e., permit the deduction of) that anomaly. But (if we accept general relativity as true) that phenomenon may be explicable by a different hypothesis, more fundamental than Newton's. It is possible that other exceptions to Newton's laws have been found; yet the Newtonian laws of gravitation are not false; they are as true today as ever; we can now see that they do not apply to all cases, only to a restricted field in which velocities are low. The negative instances show that they are a limited universal, whose limits we are just recognizing in the establishment of a more general universal. Thus the search for negative instances, while important where a generalization is not evidenced by a wide range of verified deductions, becomes unimportant when we are dealing with a hypothesis that gives a wide range of verifiable deductions. We may then conclude that, in proportion to the wideness of the range in the verification of a hypothesis, the search for negative instances becomes unnecessary to substantiate the truth of that hypothesis, and that, when a hypothesis is widely enough verified, an exception, when found, can at most only limit the application of a hypothesis; it may be explicable by a supplementary hypothesis, but it can never overthrow a rationally verified hypothesis.

There are cases, however, in which a single exception is able to overthrow even a rational generalization. In such cases it can be shown that no supplementary hypothesis is able to explain the exception. Analysis of such cases will nearly always reveal that it has been possible to verify a hypothesis incompatible with the original generalization. For example, the genuineness of a clause in a will can be overthrown when it can be shown that the words of the clause were written some time subsequently to the rest of the will, in that certain letters of the clause were superimposed upon others in the will at a period considerably later than the writing of the will, giving them a different appearance from the original writing. Here no supplementary hypothesis will save the genuineness of that clause. In this case what has really occurred is that we have verified a hypothesis which is incompatible with the hypothesis of the clause's genuineness—that the clause is later and so spurious. The proof of the latter hypothesis does not depend upon the appearance of the letters in the clause alone, but also upon a general law—letters written considerably later than the original writing and that cross the original writing have precisely the same sort of appearance that is found here. We may then conclude that supplementary hypotheses are useless when an incompatible hypothesis can be substantiated.

Such conclusions are not, however, sufficient. The importance of the exception for logical theory requires a more careful examination into its logical function. In most cases of induction, there sooner or later appears at least an apparent exception to the consequences of the hypothesis, which must be dealt with in some way. This exception may be apparent but not real, it may be explained by a supplementary hypothesis, it may indicate the limits of a limited universal, or it may overthrow the generalization.

In the first place, some exceptions are shown to be only apparent exceptions, and are really deducible from the law. Newton himself admitted that the motion of the apsides of the moon's orbit was irreconcilable with his law of gravity; but Clairaut later showed, by a more complete mathematical analysis, that the moon obeyed Newton's laws (i.e., the observed positions of the moon may be deduced from those laws). Such apparent exceptions as this can usually be removed by a wider deduction of the consequences of a hypothesis. It must be remembered that, while a deduction, after it is made, may appear self-evident, it sometimes is one of the severest and most arduous of tasks to discover a deduction that has not been made before.

In the second place, the exception may be accounted for by (i.e., deduced from) a supplementary hypothesis consistent with the original hypothesis. A hypothesis that needs such supplementation may well be mistrusted. Yet there are very many cases in which a supplementary hypothesis is necessary and valuable. When Galileo enunciated his law of falling bodies, he was confronted with the fact that light objects do seem to obey the supposed Aristotelian law that the velocity of fall is proportional to the weight of the object. Galileo proved his law true for heavy objects, and explained the behavior of light objects on the basis of a supplementary consideration, the resistance of the air.

A supplementary hypothesis can only be logically accepted when it can be shown that this new hypothesis is widely verified: when it leads to *other effects* that are independent of the first hypothesis and the phenomena that it primarily seeks to explain. Galileo could have pointed to the fact that the resistance of the air is exerted in other ways, in the wind, sailing-vessels, storms, etc., as a proof that his supplementary hypothesis was justified. When the air pump was later invented, it was found that a feather and a ball of lead fall with the same velocity in an exhausted tube. The discovery of the planet Neptune was likewise due to the formation of a supplementary hypothesis. After the planet Uranus was discovered, its course did not agree with that calculated from Newton's laws; Newton's laws had been verified so widely that scientists had not thought of abandoning those laws on account of an apparent exception; instead, they added the supplementary hypothesis that there is another planet in addition to the six which had been discovered; this supplementary hypothesis was verified when it predicted an additional phenomenon of observation—the perception of a telescopic star that changes its position. The rotation observed in the perihelion of the planet Mercury is even more instructive; that rotation proved greater than the amount deduced from Newton's laws; the hypothesis of an additional planet was again invoked, but this supplementary hypothesis failed to enable the deduction of an independently verifiable phenomenon, that is, the planet was not observed; so the hypothesis was not accepted. Yet Newton's laws were not discredited, since they had already been widely verified, and it remained for Einstein's general theory of relativity to attempt to account more exactly for the discrepancy.

A supplementary hypothesis is thus justified only when it can itself be verified by a wide range of deductions, which include other phenomena in addition to those it is invoked to explain. It is quite on a par with other hypotheses as regards the criteria necessary for verification, and requires an equal strictness of testing for its acceptance, i.e., it must have consequences that can be unambiguously shown to be due only to this hypothesis.

The form of hasty generalization that is produced by a violation of this rule is called the formation of an *ad hoc* hypothesis. The classic example of the *ad hoc* hypothesis is to be found in the development of the Ptolemaic theory of epicycles. As the observations of the motions of the planets became more precise, Ptolemy's theory had to be supplemented by additional epicycles or other machinery, to explain (permit the deduction of) the observed phenomena. These additional hypotheses were clearly *ad hoc*, in that they explained nothing more than the phenomena for which they were invoked. Similarly, the hypothesis that water rises in a pump because "Nature abhors a vacuum" had to be changed when Galileo noticed that a pump would not draw water more than 33 feet; he taunted his opponents with assuming the *ad hoc* hypothesis that "Nature abhors a vacuum only to 33 feet!" Any hypothesis that needs to be constantly altered and refitted to meet new facts is very likely to be an *ad hoc* hypothesis. On the other hand, we cannot say that, in the complex situations of modern science, changes or supplementations of a hypothesis always render it false; supplementary hypotheses are often justifiable. For example, variations in the observed times of stellar transits were explained by the supplementary hypothesis of the "personal equation," which was confirmed by psychological experiments on reaction times. A hypothesis may originate as an explanation of an apparent exception, and be confirmed by other evidence; but unless it does so, it cannot be accepted.

In the third place, an exception to the law may result, not in disproving the law, but in limiting the range of its applicability—the law is found to be a limited universal.[1] The Michelson-Morely experiment finally did that for the Newtonian physics. The indication of such a state of affairs is that the hypothesis which explains the exception includes all that the previous law included—it explains the exception, the previous law, and more facts in addition. This "more" is necessary in order to be sure that it is not an *ad hoc* hypothesis.

In the fourth place, the exception may result in overthrowing the generalization entirely. Such an overthrowal occurs in most cases only when the generalization is an empirical one; it can not occur when a hypothesis has become established as a rational law. In proportion to the wideness of verification of the generalization, such destructive negative instances do not occur. Whewell, the historian of science, writes, "No example can be pointed out, in the whole history of science, so far as I am aware, in which this Concilience of Inductions [his term for what we have called wideness of verification] has given testimony in favor of a hypothesis afterwards discovered to be false."[1]

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[1] The limited universal will be discussed in chap. viii, § 1.

Apparent exceptions to this rule can be usually explained by a failure in the original verification. Instances do occur in which a hypothesis seems to be somewhat widely verified and yet collapses at the touch of a negative instance. We take for example the forgeries of medieval manuscripts executed by such a clever forger as Constantine Simonides. Such a forgery, to be successful, has to meet a great many tests—not only must the style of the period that the manuscript is supposed to belong to and the type of writing of the place and period be imitated, but old paper with its appropriate watermarks, or parchment carefully stained, must be provided and duly discolored; the ink must be of the right tint and appearance and the writing must be firm and boldly drawn, not falteringly traced. Furthermore, numberless interdependent facts of the particular time and place have to be implied in the manuscript; usually facts have to be added which are not to be found in existing authorities, in order to give an air of original knowledge. If the manuscript is genuine, it can be deduced that all these characteristics are found. Yet Simonides succeeded in deceiving some of the cleverest critics in England and Germany. His supposed Greek palimpsest record of Egyptian kings by Uranius deceived the Prussian Academy and was almost published by the Oxford Press, when it was found that the "ancient" writing of Uranius was on top of the "later" twelfth-century writing, as could be seen by the microscope, and that the Greek was far from correct. Here were two real exceptions to the hypothesis of the genuineness of the manuscript, which immediately disposed of that theory.[2] The coincidence between the deductions from the hypothesis and the facts of observation was wide, but that coincidence was confined to only one field—the field of the facts to be learned about such manuscripts from a careful study of existing manuscripts; the verified deductions did not extend to different fields; consequently, the hypothesis was overthrown by a few exceptions. The disproof of a historical novel which is supposed to be history would likely exhibit similar characteristics.

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[1] *Novum Organum Renovatum*, p. 90.

[2] F. Madan, *Books in Manuscript*, chap. ix, sec. 6.

Thus we see that the problem of accounting for an exception to a hypothesis is not by any means simple, and that the nature of the previous verification of the hypothesis must be and usually is taken into account in dealing with such an exception. In many cases negative instances do not destroy an inductive generalization. A negative instance may destroy an empirical law, but not a verified rational law; the misunderstanding of the nature of induction by logicians is largely due to a confusion of the two.

§ 17. It is pertinent, at this stage of our inquiry, to ask when a hypothesis may be considered to be established as false. Violation of the logical requirements for the establishment of a hypothesis does not show its falsity, but merely its logical weakness. The surest proof of falsity is when another hypothesis, incompatible with the first one, is established as true. The example of the spurious clause to a will, given in the previous section, is a case in point. The proof of positive falsity requires just as much evidence and logical strictness as positive truth does.

Sometimes we wish to disprove a hypothesis when no incompatible view has as yet been established. In establishing the correctness of one view, we do not require proof of the positive falsity of others, merely proof of their weakness, thereby throwing the burden of proof upon their advocates. Men spend little time upon views that seem likely to prove false. If a hypothesis has not as yet established itself by a wide range of verified deductions, the instancing of an experience which is incompatible with a necessary deduction from that hypothesis and from any supplementary hypothesis which has as yet been proposed is sufficient for purposes of argument, when some positive proof of a rival hypothesis can be adduced. When such positive proof cannot be given, the mere showing that an exception occurs is insufficient to establish the positive falsity of any view. In order to establish such positive falsity, it must be shown that the deduction concerned is a necessary one, i.e., that it follows with logical exactness from the original hypothesis; that no supplementary hypothesis, compatible with the original hypothesis, can permit the deduction of results agreeing with the experience; and that the original hypothesis is not widely verified. The second of these requirements is undoubtedly very difficult of attainment, and yet it is attained in some cases. An example is Count Rumford's disproof of the phlogiston theory, which showed that a seemingly infinite amount of phlogiston could be extracted from iron by friction. In this and similar cases, it is possible that there might be an unknown explanation of this phenomenon, quite compatible with the original hypothesis; yet the flexibility of the human mind is such that, given a body of active investigators and sufficient time, if the required explanation does not appear, there is positive presumption that such an explanation does not exist. In this fashion, the lapse of time is perhaps the surest proof of the falsity of a theory, next to the establishment of the truth of an incompatible view.

§ 18. Who is to decide when there is enough evidence to warrant the acceptance of a hypothesis as true? That decision is given by the group of those who are unprejudiced and who have studied the subject and its proposed solution sufficiently to understand it thoroughly.

Modern science has come to be suspicious of the genius or individual wise man as the ultimate court of appeal for truth. Geniuses have their prejudices and foibles, just as do ordinary men. Good judgment, which is the faculty chiefly required for our purpose, is not confined to geniuses; it is often found to a higher degree among ordinary men. The genius is needed to discover the illuminating hypothesis and to make the crucial deductions and observations. The rest of us must wait for him to lead the way; but once the path is blazed, we may all follow and understand his reasoning; when we understand his reasoning, we can judge it. Geniuses have made as many mistakes as successes. Kepler published the *Mysterium Cosmographicum;* Newton wrote second-rate theology, even including it in his *Principles* and *Opticks;* he clung to the corpuscular theory of light: false announcements of scientific discoveries, even by the greatest of scientists, are common.

On the other hand, the mythical "ordinary man" is often unable to judge of such matters, especially when they concern the natural sciences. He is unacquainted with the terminology used; he frequently fails to understand the import of statements made; methods of deduction and modes of exact observation are a mystery to him; he does not understand in just what direction mistakes are likely; and he is more than likely to be suspicious of the whole procedure and prejudiced against anything new. His failure is a failure to understand the matter.

Such a statement as that of Karl Pearson, "The touchstone of science is the universal validity of its results for all normally constituted and duly instructed minds,"[1] is also unable to help us. In the first place, all "normal" men cannot be equally good judges of a complicated scientific induction. Shall we say that the final proof of Einstein's general theory of relativity is its validity for all "normal" minds? We would hardly accept them as worthy judges any more than Socrates did! The degree of knowledge that a person has about a matter such as relativity has a great deal to do with the worth of his judgment. In addition, there is the further objection: How can we define "normal" without making our criterion circular? Abnormality is simply difference from the usual opinions and practices of men; are we then to say that when Copernicus or Galileo challenged the whole world with their theories of astronomy or of falling bodies they were abnormal? When a person asserts that he is emperor of the world we put him in an asylum or carefully guard him; those whose opinions have differed from that of the multitude have always been looked upon as queer. We must avoid the term "normal" in our criterion.

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[1] *Grammar of Science*, p. 73.

The true judges are those who are unprejudiced and who are sufficiently acquainted with the particular subject under consideration to understand it thoroughly. Prejudice naturally prevents anyone from rendering a fair verdict. Given a lack of prejudice, thorough understanding alone is necessary. Science does not restrict its judgment seat to a limited few who may be considered infallible, and to whom the rest of us must go in order to know the truth. Science, which is the search for knowledge, in whatever field it may be found, physics or philosophy, is fundamentally democratic. The modern world has flung the gates of science wide open, so that all who wish may enter—but the steps leading up to those gates are often so steep and the elevation to be attained before arriving at the gates is often so high that few do actually enter. Patience and industry are necessary to surmount the ascent: some fail because they lack intellectual power; others drop away through weariness of the flesh; but to all who attain the threshold, entrance is free and unhindered. To those who have mastered the method and subject matter of science is given the privilege of sitting in judgment upon the discoveries of her geniuses. Scientific truth does not make its way by an appeal to any infallible pope or revelation, but by its own inner persuasiveness. Unless a scientific hypothesis appeals, as logically verified, to anyone who unprejudicedly and maturely considers it, after fully understanding the subject to which it refers, it cannot be said to be true.

Inductive truth does not appeal to a majority decision of any jury. The majority vote is an expedient to secure a decision upon practical matters that require immediate decision. It has justified itself as a practical expedient, but it has been by no means successful in obtaining the truth—majorities have often been wrong; nevertheless, the compulsory power of truth has in the end usually brought the majority around to it, when the matter was not too recondite and prejudice did not befog the issue. Majority rule is justified because of the fact that, with time, prejudices usually disappear and the truth is recognized. "Truth will out." But the lapse of time alone cannot guarantee that the majority will have the intelligence to decide complicated matters.

Science can wait as long as necessary for truth to evidence itself; it is not disturbed by the exigencies of practical life. Science waits for a unanimous verdict. Yet the continued arbitrary opposition of a single individual or a group of individuals does not change the verdict of science—she cares not for the opinion of the multitude, but only for that of the wise—those who are unprejudiced and who understand. As a matter of practice, science has found it expedient to take the preponderance of opinion in a group rather than the judgment of an individual; not because the judgment of a group is better than that of an individual, but because individual prejudices and ignorances can be eliminated by taking the decision of the group and neglecting that of the few who deviate from the general opinion. The Socratic ideal of the wise man who alone can judge is made a reality in the group of those who have the ultimate decision upon any inductive hypothesis.

It is obvious that the amount of knowledge necessary to make an individual a proper judge of the truth of a hypothesis must vary with the nature of the subject at issue. When it is such a complex and highly technical matter as the theory of relativity, only a very few persons in the world can qualify as possessing the ability and knowledge to really understand the matter. On the other hand, when it is such an ordinary matter as "N. N. is a suitable person for the presidency of the United States," any mature citizen (with certain restrictions, such as absence of criminal record, insanity, etc.) is supposed to be able to decide upon the matter (although it is doubtful just how far the "average citizen" does make an intelligent decision). The jury system is founded upon a similar presupposition—that any twelve unprejudiced men are able to render a correct decision upon the facts of the case (although the "law" of the case is supposed to be beyond their ability), and that, if the accused is guilty, the proof of his guilt will convince them all. How far the actual conduct of jury trials carries out the theory—whether unprejudiced jurors are actually secured by our present methods of selection, whether all the facts are presented and considered, whether adequate search is made for negative instances, etc., whether prejudice is allowed to bias the verdict—all these considerations are matters of practice, and do not affect the theory therein implied—that the truth will evidence itself to *any* unprejudiced man who understands the case. The democratic system of choosing public officials, the unanimous jury trial, the board of arbitration, these and many other practical devices of political and commercial life are implications of the method of induction, constant use of which has led practical men to an intuitive understanding of its implications, even when it has not been thoroughly comprehended.

The tenacity with which these institutions have been clung to, in spite of their failures and the many attacks which have been made upon them, shows that underlying them there is something which is fundamentally sound. The theory of rational induction gives the reason for many of our most cherished practical devices and institutions.

The fundamental justification for political democracy is that it corresponds roughly to this inductive criterion—that hypotheses as to policies to be pursued in politics, as in science, should be judged by the court of those who understand the subject. But, in modern democracies, the state has become so large and life so complex that often "those who understand" about political measures are actually only a small proportion of the electorate. The failures of democratic institutions have arisen because their members have failed to meet the standards for making an intelligent judgment upon an inductive question—voters have been asked to pass upon complex matters of public policy whose ramifications they ill understood, or they have failed to acquaint themselves adequately with the persons for whom they voted; jurors and judges have been consciously or unconsciously swayed by prejudice in violation of their oaths; the prosecution or the defense has failed to present a sufficiently wide range of facts for the substantiation of hypotheses; time and patience have not been given to search for the additional evidence necessary to provide a wide variety of verified deductions—these and other defects have prevented these institutions from functioning as they theoretically should, so that we have become impatient with them. The problems of democracy are at bottom largely problems of getting citizens and officials intelligent enough and interested enough to judge its problems in accordance with the requirements of rational induction.

There is then no certain list of judges who shall decide the truth of any inductive hypothesis. The constitution of the jury of ultimate appeal varies with the nature of the question at issue and with the character of the individuals. Membership in that jury always remains open to anyone who will prepare himself to make an adequate decision. Scientific truth is democratic, in that it is open to all; but it is aristocratic, in that only those who prepare themselves adequately may judge of it. The number of the intelligent and the unprejudiced is always relative to the situation and the knowledge of the individual.

It is furthermore true that everyone has a right to an opinion on any matter in proportion to the amount of knowledge he has on that subject. While a considerable amount of knowledge is necessary before anyone can be an "authority" and feel rightfully confident that his decision on any matter is sufficiently worth while to be of value to other persons than himself, yet everyone, in the proportion that he knows about any matter, has the right and the duty of making up his mind for himself. That decision may be based upon his estimation of various "authorities," or it may be based on a survey of the evidence. But no one has any justification for saying, "This is true because I say so, and you *must* accept my word." Truth compels only the wise to accept it, and they need no compulsion; he who rejects the truth does so at his own peril.

This uncertain and inexactly defined criterion of membership in the jury that decides upon the correctness of inductive proof is just the criterion that is being constantly used. The justification for such lack of exactness is that exactness is not necessary. It is usually possible to have more than the minimum amount of knowledge needed; in fact, we never trust an "expert" or a teacher unless he has more than enough knowledge for our needs. In scientific matters, appeal is made to the "scientific world" or to some such inexact body of the wise. The "expert," who is called in by practical men, is the individual who "knows," who has made himself a member of this jury in cases in which, because of the difficulty of the problem, ordinary people are excluded. This appeal to the "expert" is not undemocratic but thoroughly consistent with our inductive theory. Furthermore, the verdict of the expert is not slavishly accepted, for everyone, in proportion to his knowledge, has a right to decide on any matter. Even ordinary men know a little with reference to health and disease, and physicians, even specialists, sometimes pretend to know more than they really do. So the ordinary person does not always accept the decision of the expert, be he "efficiency expert" or physician; he uses his own judgment because he too, in a measure, belongs to the jury of those who know, and has a right to exercise his own judgment when the matter concerns himself. Counting votes to determine the truth of the hypothesis of biological evolution is ludicrous, and the appeal to the judgment of unprejudiced biologists is sound, but everyone has the right to make up his own mind intelligently for himself on such a subject.

§ 19. We shall close this chapter with a few examples of the verification of hypotheses, and shall choose them, by preference, from fields of investigation in which mathematical deduction is not used, since mathematical deduction is well known, whereas other types of deduction have been comparatively neglected.

Harvey's demonstration of the circulation of the blood is a good case of inductive verification. Before his time it was thought that the blood simply oscillated, or moved back and forth like a pendulum. This theory had never been really verified, but it was simple and easily understandable. While working in Italy, Harvey probably saw the valves in the veins, which had been discovered by Fabricius. On his return to England, he realized that they could be of use only by permitting the flow of blood in one direction, while preventing its movement in the opposite direction. He thereupon set to work to verify his hypothesis of the circulation of the blood. From that hypothesis he deduced a great many different circumstances. There ought to be some organ to propel the blood —Harvey studied the action and construction of the heart and showed that it acted as a pump. Furthermore, from the hypothesis of the circulation of the blood, he deduced that it must be possible to find a circular path for the blood. With great patience, he traced the paths of the arteries and veins, and showed that the arteries kept dividing and subdividing, with their branches getting smaller and smaller, until they lost themselves in the tissues, while the veins arose as small tubes in these same tissues and kept uniting and increasing in size until they reached the heart. Because the microscope had not yet been invented, he could not show the connection between the arteries and veins, but his hypothesis showed itself so powerful in explaining other phenomena that he felt confident in assuming that connection, even though it could not be perceived. Harvey's proof of the circulation of the blood had justified itself in so many different fields that, when the microscope was invented, Malpighi's observation of the blood in the capillaries was really superfluous, although, had he not been able to make that observation, Harvey's hypothesis would have received a severe setback. It is to be noted that Harvey's proof, though complete, was not deductive—he did not derive the circulation of the blood from logically prior considerations; he merely showed that the construction and functioning of the heart, arteries, veins, valves, and their parts, could be deduced from the hypothesis of the circulation of the blood. His demonstration[1] was not a fallacy of affirming the consequent, because his deductions were so various and the correspondence between his deductions and observations so complete that no other hypothesis could have satisfied the conditions.

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[1] The term "demonstration" is used by scientists of an inductive proof that is complete and compelling; Newton is responsible for the currency of this meaning of the word; it has by no means been confined to purely deductive proof.

Professor Tyndall has given another excellent example of rational non-mathematical induction in his geological account of "The Parallel Roads of Glen Roy."[1] We can merely give a brief analysis of his argument. These "roads" are flat, level shelves occurring on the sides of three valleys in a remote part of Scotland. In Glen Roy there are three such shelves with a vertical distance of about a hundred feet between them; they are perfectly level; they wind in and out of the sides of the mountains with no variation of their contour level; and they are at the same height on opposite sides of the valley. The lowest shelf alone is continued in a second valley, Glen Spean. In a third valley, Glen Gluoy, there is one only shelf, at a higher elevation than any in Glen Roy. The country people propounded the hypothesis that these shelves were roads which were made for the chase. This hypothesis permits the deduction of level open spaces, and so it was accepted by the earliest investigators. But it accounted for nothing more (it was a hasty generalization), and it failed to permit the deduction of any reason for such a phenomenon as the parallelism of the roads on opposite sides of the glen. Yet so long as no wider deduction from the hypothesis was made, this failure could not be seen.

Dr. MacCullock later suggested that these "roads" were margins of ancient lakes, but failed to explain why they should have been formed at three levels. Sir Thomas Dick-Lauder followed the single shelf in Glen Gluoy, which was at the highest level of all, and found at the head of the glen a watershed at the same height as the shelf, with a collateral valley leading down to Glen Roy. He likewise followed the highest of the three shelves in that glen, and similarly found a watershed at the head of that glen at the same height as the shelf, with a valley leading down to Glen Spean; then he descended to the

[1] *Fragments of Science*, Vol. I, chap. viii.

lowest shelf in Glen Roy, found it led into Glen Spean, and at the head of that glen found a watershed at the same height as the shelf. His hypothesis had now framed itself in his mind—if the mouth of Glen Gluoy were blocked, a lake would be formed, whose surface level would be that of the watershed at the head of that glen; the action of the water on the friable drift overspreading the mountains would cause the shelf to form. A similar barrier at the mouth of Glen Roy would cause the formation of the highest of its three roads; when that barrier was removed, but a barrier at the mouth of Glen Spean still remained, the lowest road would be formed. But how were these barriers formed? Sir Dick-Lauder saw masses of detritus in certain portions of the glens, and assumed that the barriers were formed of that material by some unknown convulsion. To account for the middle road of Glen Roy, he introduced the supplementary hypothesis that at a certain point in the breaking down of the dam, a halt occurred, forming the second road.

Mr. Darwin called attention to this supplementary hypothesis, and showed that it implied that the levels of the watersheds did not constitute an essential feature in determining the phenomena of the roads, i.e., that the middle road constituted an exception to the theory and that the supplementary hypothesis was *ad hoc* and not really consistent with the main hypothesis. He proposed the hypothesis that the shelves were caused by the sea; that the region had been submerged and subsequently upheaved, and the shelves represented pauses in the upheaval. But from this hypothesis there could not be deduced any reason for the shelf in Glen Gluoy being 20 feet higher up than the highest shelf in Glen Roy, or for the absence of the two lower shelves in Glen Gluoy or the other glens. Thus neither Mr. Darwin's hypothesis nor that of Sir Dick-Lauder permitted the complete deduction of the phenomena, and neither could be considered established.

Some years later, Mr. Milne-Home again explored Glen

Roy. He found a lateral glen near the mouth of Glen Roy, at one head of which was a watershed at the same level as the middle shelf in Glen Roy. The hypothesis was then changed—when the barrier in Glen Roy was some distance inside the glen, the highest shelf would be formed; when the barrier was removed to the mouth of the glen, the level of the lake would sink to the height of the middle shelf; when it was entirely removed, but still existed in Glen Spean, the lowest shelf would be formed.

But what could have caused the barrier? From the hypothesis that it was composed of detritus is deducible the fact that such barriers, more than 230 feet high, must have left traces behind them; yet such traces were not found. There is detritus in Glen Spean, but not where it should be according to this hypothesis. The supplementary hypothesis that the removal of the detritus was caused by glaciers is untenable, for that hypothesis implies that the glaciers would also have removed the shelves, and at one place a shelf winds around a great mound of detritus. This supplemental hypothesis failed to explain anything new, and it had important exceptions. The part of Sir Dick-Lauder's hypothesis which stated that the barrier was caused by detritus failed to permit the deduction of the observed facts, and it permitted the deduction of consequences which were not found.

Agassiz later visited the place and pronounced the barriers to have been constituted of ice, due to glacial action. He was the genius who first suggested the correct hypothesis, but he did not confirm and establish it; the thorough testing and confirmation of Agassiz' hypothesis was due to others. This hypothesis was found to permit the deduction of all the phenomena observed, and to predict new ones. It predicted glacier markings—they were found. The mountains south and west of Glen Spean, of which Ben Nevis is the monarch, were found to be exactly so situated as to induce the vapor laden winds from the Atlantic to give up their moisture. The proof of this subsidiary hypothesis is found in the high precipitation in those regions today. In a glacial epoch, this rain would fall as snow, and glaciers would be formed. The proof of such a statement is to be found in thermodynamic laws and in the impressive traces of glacial action in the portion of Ireland that is similarly situated with reference to the winds from the Atlantic.

We find an apparent exception when we reflect that this hypothesis requires that the glaciers should occupy the mountains to the south of these three glens, but that the glens themselves must be free from ice and full of water. This exception is, however, only apparent, for the hypothesis shows itself to be able to deduce exactly that phenomenon. The mountains to the south are higher than those to the north; they would relieve the winds of their moisture and, consequently, there would be comparatively little precipitation north of the glens. This deduction from thermodynamic laws is confirmed by the differences of precipitation locally observed, and by the observed difference in power of snow collection by the mountains north and south of the glens. Tyndal observed the mountains north of the three glens with hardly a fleck of snow, but those south still loaded with heavy swaths and patches.

These and other deductions from the hypothesis are to be found in Tyndal's account. To illustrate the action of the ice barriers, he had a topographical model of the glens made, and showed, by the successive removal of dams of glass, how the three levels of the lakes in Glen Roy would be produced by the gradual retreat of the ice barrier. The reader is referred to Tyndal's original account for further illustration. It is an admirable example of rational inductive proof by the use of non-mathematical deduction, with a successive series of hypotheses, gradually approximating the truth, with supplementary hypotheses, and various types of exceptions. It illustrates beautifully our position that the rational proof of a hypothesis consists in the verification of widely different deductions from that hypothesis.

§ 20. In summary of this long chapter, we observe that there are two cases in the verification of an induction; when deduction is used and when it is not used. The first case, that of the immediate description, was found to be very simple—verification is a matter of exact mental comparison of the experience with its description when they are held in mind together—but such inductions are not the larger inductions that matter most. These are found in the other case, in which deduction becomes a highly important part of the verificatory process—the rational induction. Such an induction may only be said to be verified when it can be shown to be the only hypothesis which can permit the deduction of the observed facts of immediate description; that criterion is fulfilled when deductions from the hypothesis have been carried out into a sufficiently large number of diverse fields, and have enabled the deduction of precisely the phenomena observed in those fields. As subsidiary criteria we found that the requirement of possibility of verifiable deductions, the principle of the non-verification of otiose elements in a hypothesis, the principle of superimposition, and prediction, were valuable aids. We found that in cases in which the wideness of verification is in question, a true hypothesis must permit of the deduction of a wider range of verified deductions than the nearest possible alternative hypothesis. The ultimate judges of truth and falsity are the group of those who are unprejudiced and who understand the matter thoroughly. An exception to a rational inductive law was rarely found to overthrow it, but usually to be explained as only apparent, by a supplementary hypothesis, or as indicating the presence of a limited universal. This last concept, together with some others that we have met in this chapter, must next be discussed.

CHAPTER VIII

THE LIMITED UNIVERSAL AND OTHER FEATURES OF INDUCTIVE PROOF

§ 1. The limited universal, which was only referred to in the previous chapter, is sufficiently important to justify a detailed discussion. Theoretical logic has been very much encumbered because of the ideal of the (absolute) "universal," which must be true without exception. As a matter of fact, there is no simple proposition known that is true without limitation, and there seems to be no reason to think that there can be any such proposition. Even such an eternal truth as $2+2=4$ fails to hold when applied to fluids. It may be possible to find some sort of contradiction in every separate field of thought, which could be eliminated by the law of non-contradiction; but the sort of contradiction that is found in one field does not apply to another[1]—the "law of non-contradiction" is a term for a set of similar principles, none of which apply universally, but each only within a certain limited range. In the modern discovery of the "universe of discourse" there is an implied recognition of this general principle. DeMorgan remarked that we do usually think and argue in a limited universe or sphere of notions, even when it is not expressly stated.[2] The statement that every proposition refers to a universe of discourse implies that that proposition is only true within that universe of discourse. Because we wish to preserve the indefeasibility of our universals, we do not say that a universal is false outside of its universe of discourse, but that it is not pertinent—by a legal fiction we save the absolute universality of our universal. If, then, a universal is only true within limits, it is important to know what are those limits and to consider the limits as well as the universal.

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[1] Cf. chap. ii, § 9.

[2] *Syllabus of a Proposed System of Logic,* secs. 122–23; quoted in Jevons, *Principles of Science,* p. 43.

In the development of science, the phenomena due to the limits of universals are worthy of careful attention. It often happens that a universal proposition, which has been established by the method of hypothetical induction, appears to be established securely, but a phenomenon is discovered that the established law seems unable to account for; finally the exception and the old law are found to be accounted for by a new and more inclusive law, and the former law is shown to be true only within limits. Sometimes the discovery of the more inclusive universal is made without the previous appearance of an exception.

Perhaps the most striking example of this phenomenon is to be found in the development of physical theory from Newton to Einstein. Newton's theory of universal gravitation was widely verified beyond all doubt, in spite of the unexplained irregularity in the motion of the planet Mercury. After the irregularities of Uranus had been accounted for by the discovery of a new planet, and the irregularities of the moon by a further analysis of the elements of her motion, it seemed quite probable that the motion of Mercury could be accounted for by the influence of a new planet or the matter of the zodiacal light. But these supplementary hypotheses failed to provide any new phenomena that could be verified, so the motion of Mercury remained unaccounted for (i.e., Newton's laws failed to deduce the observed motion of Mercury, and no supplementary hypothesis could be verified that would do so). Furthermore, when the velocity of light was found to be finite, it was deduced from Newton's laws that by measuring the velocity of light in different directions, it should be possible to discover the absolute velocity and direction of the earth's motion. But the crucial Michelson-Morley experiment and others similar to it failed to show the difference of velocity predicted by the Newtonian laws. Here was another exception to the well-verified laws of Newton. Physics was in a quandary. Various supplementary hypotheses were suggested to account for the discrepancy, none of which were successful in being established.

Then Einstein propounded his theory of relativity. For low velocities, Einstein's formula reduces to a form that is practically identical with Newton's theory—thus the Newtonian laws are included as special cases of Einstein's laws; Newton's laws are therefore not false, but true, with the restriction that they apply only to cases in which velocities do not approach the velocity of light. Hence from the Einsteinian formula there are deducible all the phenomena that are deducible from the Newtonian formula; but the Einsteinian formula also permits the deduction of those crucial phenomena that Newton's laws failed to account for—the Michelson-Morley phenomena, the dependence of mass on velocity, etc. From the general theory of relativity, Einstein deduced the observed secular advance of the perihelion of Mercury, and, in addition, he predicted two new phenomena that had not been previously observed—the bending of light-rays in a gravitational field to an amount approximately twice that deducible from the Newtonian formula, and the Einstein shift-effect, a shift of spectral lines toward the red, in light originating from atoms in a strong gravitational field. These two phenomena are said to have been observed, and consequently, the general theory of relativity has been considered to be verified. Whether the theory of general relativity produces a larger number of verifiable deductions than any alternative hypothesis and whether any other hypothesis may not be able to explain the same phenomena, are problems that, however, need serious consideration.

The precise relation that holds between the Newtonian and the relativity theory should be noted. The relativity theory does not contradict any elements in the Newtonian hypothesis which are actually used to make deductions, except under the special circumstance of high velocities. With moderate velocities, the Newtonian laws remain unaltered. Absolute space and time have been dropped, but those concepts are now recognized to have been otiose in the Newtonian theory, and consequently, unverified. The relativity theory includes everything that was verified in the Newtonian theory and more besides. The Newtonian laws remain as special cases of Einsteinian laws, true under certain restrictions. Newton's formulation of universal gravitation is found to be, not false, but a limited universal, true under certain circumstances only. The restricted theory of relativity is likewise a limited form of general relativity. It is in relations of this sort, in which one generalization includes (permits the deduction of) a larger number of phenomena than the other, as well as all the phenomena of the other generalization, that the more inclusive one may be called a more general universal, and the less inclusive one a limited universal.

The difference between these sorts of universals can also be illustrated by comparing Kepler's laws with Newton's universal gravitation. Kepler's first law, that a planet's orbit is an ellipse, with the sun in one focus, has been shown to be limited —it is only true when there are two bodies in the system and no more; the introduction of a third body causes "perturbations," and the orbit of the planet varies from an ellipse. Newton's hypothesis, on the other hand, not only tells why planets should move in an ellipse, by deducing the elliptical figure of the orbit from the hypothesis of universal gravitation, but also enables the deduction of those very "perturbations" that threatened the overthrow of Kepler's law.

In like manner, Harvey was greatly disturbed by his inability to discover why the blood should make such a rapid round throughout the body; Harvey's law of the systemic circulation of the blood was a limited universal; the *why* was not found out for generations, until the discovery of oxygen and the gradual elucidation of the combustion involved in the renewal and cleansing of the system by the blood enabled the formation of a more general universal, which includes Harvey's generalization as a deduction. In this case, the less general universal was not shown to fail to deduce any case, but to contain only part of the truth, and to need supplementation.

The phenomena due to the limited universal have occurred repeatedly in the development of science; in fact, the progress of science may be said to consist in the discovery of more inclusive generalizations for previously known laws and facts more than in the mere discovery of new facts. Some elements of the old formulation have sometimes been found to be otiose and false, but the old formula, because it has been verified, is not rejected; it is embodied in the new one as a special case. At the beginning of modern chemistry, it was very soon found that physical bodies acted as if they were composed of little unchangeable units of matter; the atomic theory was used to explain (permit the deduction of) those phenomena. More recently, physicists have found a simple numerical relationship between the atoms of different elements; consequently, the hypothesis has been advanced that the supposedly "indivisible" atom is composed of a number of minute positive and negative charges of electricity (electrons); the chemical behavior, atomic weight, etc., of various chemical elements have been shown to be largely deducible from the hypothesis that each element is composed of a definite whole number of such positive and negative electrons. Certain atoms were found to break up into simpler ones in accordance with this new hypothesis, and one scientist has even been able to capture a single minute electron and to measure its electrical charge. Thus the old atomic theory was discovered to be a limited universal, which is included in the newer hypothesis that atoms are composed of these electrically charged bits of matter. Now the quantum theory is suggesting that this hypothesis is likewise a limited universal, and must be included in a still more inclusive theory.

The conclusion which we draw (induce) from this phenomenon is that any generalization or hypothesis whatsoever, no matter how well it seems to be established, may later be shown to be a limited universal, and to be true only within limits. We furthermore see that an exception, even though it is a real and not an apparent exception, may not overthrow a hypothesis, but may merely indicate that the hypothesis in question is a limited universal. Modern science has recognized this fact in many instances. It has been shown that the ordinary laws relating to the behavior of substances do not apply for temperatures near absolute zero. It is possible that the Newtonian laws of motion do not hold within short distances of the nucleus of an atom. Most empirical laws, such as Weber's law in psychology, are known to be limited in their application. We must recognize that not only an empirical law but even the best established of rational laws may also be only a limited universal.

This phenomenon of the limited universal and its later inclusion in a more general statement seems to point to a monistic conception of the world—that the whole universe may ultimately be stated in a single formula, and be deducible from that formula. While such may be the case, we must remember, nevertheless, that such a theory is, as yet, a mere speculation, and that the phenomenon of the limited universal and the superimposition of one generalization upon another is insufficient evidence for the monistic hypothesis. While it might not be necessary to complete the synthesis and state the Absolute for an inductive proof of this hypothesis, yet a much wider range of evidence is necessary before either the monistic or the pluralistic hypothesis can be verified.

It must further be remembered that nothing can be deduced from one proposition alone, and that therefore the hope of reducing the universe to a single statement, or of stating all natural laws in one mathematical equation, is chimerical. We have previously seen[1] that it takes at least two propositions, together with a method of deduction, to make any significant deduction; and that therefore, when we seem to have included a whole physical or mathematical system in one equation or proposition, the statement is only apparently simple. The equation cannot be developed without introducing a new factor, such as a parameter; and the development cannot be at random, but must be in accordance with certain laws: when we have stated all that is presupposed in our single equation and its development, we have not a simple statement but a highly complex one. The simplest hypothesis that can be significantly framed is composed of a group of propositions, not merely one.

Another consequence of the limited universal is that, after a science has developed, we can see that each hypothesis which appeared true in the past, but was shown to be erroneous by the progress of science, nevertheless contained a certain amount of truth, and that this truth was taken up by the next larger generalization. As an example, take the successive astronomical theories. The primitive view that the sky is a dome or firmament supported upon a flat earth contains very little truth, but it at least makes a distinction between the fixed stars and the planets, which move about among the stars. The view of Eudoxos and Aristotle that the earth is the center of a series of concentric spheres includes the truth of the former and also arranges the planets in their correct order (except the sun and moon). The earth is a globe, and the very reason that induced Aristotle to fix the earth at the center of the universe is itself a truth—that gravity tends toward the center of the earth. Unfortunately he did not realize that this, too, is a limited universal. The next view, that of Hipparchus and Ptolemy, which explains the motion of the planets by deducing them from sets of epicycles and eccentrics, retains the truth of the former theories and has another truth in addition—that it resolves the apparent motions of the heavenly bodies into an assemblage of circular motions. Such a resolution of the unequal motions of the heavenly bodies into equable circular motions is, in fact, equivalent to the most recent and improved processes by which modern astronomers deal with such motions—they are resolved into a series of terms, or expressions of partial motions, and these terms involve sines and cosines, which are technical modes of measuring circular motion. As Whewell says,

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[1] Cf. chap. vi, § 21.

> In this sense, therefore, the Hipparchian theory was a real and indestructible truth, which was not rejected, and replaced by different truths, but was adopted and incorporated into every succeeding astronomical theory; and which can never cease to be one of the most important and fundamental parts of our astronomical knowledge.[1]

Copernicus put the sun at the center of the solar system and made the earth move, but the sun was supposed to be the fixed center of a limited spherical universe, and the planets were thought to revolve in circular orbits with uniform motion; he retained many epicyles and eccentrics to account for their positions—his system, while it retained all the truth of the preceding ones, and added to that truth, yet mixed it with much error. Kepler's discoveries, while correcting the shape of the orbits of the planets from circular to elliptical, and adding certain empirical rules for their velocities and distances from the sun, yet were only approximately correct; Horrox, in order to account for the inequalities of the moon's motion, had to represent the center of its ellipse as revolving on an epicycle. Kepler's first law is only a limited universal. Newton's formulation of the law of gravitation has been likewise shown to be a limited universal; who will show the limits of Einstein's theory of relativity?

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[1] *History of the Inductive Sciences*, I, 154; cf. also the preceding pages.

Each generalization is thus shown to be true in some respects and false in others, though the particular characteristics which are permanently true in a theory have sometimes been such as would hardly have been suspected by the advocates of that theory. It is by no means easy for any generation to see wherein the limits to the truth of its knowledge lies; that insight must often wait until the next larger generalization has been established.

This characteristic of the development of science has produced the view that there is no such thing as absolute truth or falsehood, but merely degrees of truth and falsity. Our analysis of the development of knowledge and the notion of the limited universal shows us that the conception of degrees of truth is only an appearance caused by the mixture of truth and falsity in successive generalizations; in reality there are no degrees of truth, but merely *degrees of mixture* of truth and falsehood. Logically considered, a concrete statement can only be true or false, and there is no place in logic for such a concept as "degrees of truth." Either Socrates is mortal or he is not mortal. Yet when we consider such a hypothesis as the Ptolemaic astronomy, we see that it is not entirely false, but that there are a great many true and approximately true statements included in it. From one standpoint we may say that it is false, because it is not the whole truth; but from another standpoint it is partly true, because elements in it are what we now recognize to be the truth. Some generalizations contain more of truth than others, that is, they are less limited universals; exact investigation shows only truth and falsehood in varying mixtures, no degrees of either.

Had men realized the possibility that they were dealing with limited universals, they might have attempted to determine the limits of each of these generalizations; because they supposed they had an absolute universal, they failed to investigate, and so failed to discover its limitations. The degree of absolute truth in each hypothesis might sometimes have been discovered by eliminating the otiose elements of the hypothesis, those that did not contribute to its verified deductions. Thus we see that while there is absolute truth, yet we cannot always be sure how much of reality is covered by any particular piece of knowledge, until we investigate its limits. Even then we cannot be sure that there may not be some obscure phenomenon that has escaped our search, which would therefore indicate a limit to that truth. But we can be sure that when we have verified a hypothesis in a particular limited field, it will continue to be true in that field.[1] The occurrence of limited universals shows that in the statement of a generalization, its limits should, as far as possible, also be stated.

There is, however, another fact to be noted: the wider the range of verified deductions from a hypothesis, the wider is its field, and the less likely it is that there are any far-reaching limitations to it; although we can never be sure that there are no unknown limits, since Newton's hypothesis, which exhibited an extraordinary degree of wideness of verification, has been shown to be a limited universal. But the progress of science consists largely in the substitution of wider and wider generalizations for those which are limited, hence progress consists in pushing back the limits of universals. The more advanced a science, the more comprehensive are its generalizations, so that scientific progress means that the limits of universals are pushed back farther and farther, that more and more truth and less and less error are included in them, and we may hope to arrive at a stage in which there are no more limitations to our universals, since they have reached the limit of comprehensiveness. The limited universal is then a consequence of the human need of building up its generalizations piecemeal; when the boundaries of knowledge have been pushed back and generalizations have become as comprehensive as possible, universals cease to be limited.

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[1] Cf. chap. xiii.

§ 2. While, in some cases, the formulation of a more inclusive generalization has resulted in what appears to be a simpler hypothesis, in other cases, the result has been a more complex one. We therefore need to face the problem of the correctness of the law of parsimony.

If William of Occam's statement, *Entia non multiplicanda est praeter necessitatem*, is interpreted to mean that no part of a hypothesis which is unnecessary for making the deductions on which the verification of the hypothesis depends, that is, no otiose element of a hypothesis, can be considered to be verified, we can have no objection to the law of parsimony, for we have already accepted this criterion as necessary and useful.[1] But Occam's law is not usually interpreted in such a restricted manner. It is usually made to mean something as follows: "In choosing between two rival hypotheses, we prefer the one which, taken together with all the others required for the explanation of the sum-total of phenomena, gives us the simpler view of the universe as a whole,"[2] or, as Mill said, the laws of nature "are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result."[1] This law of parsimony has been declared to be an essential part of scientific method.

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[1] Cf. chap. vii, § 12.

[2] R. H. Dotterer, *Beginners' Logic*, p. 155.

The law of parsimony has been analyzed into two principles, the principle of superimposition and the principle of simplicity. The principle of superimposition we have stated to be a description of the way in which scientific knowledge develops.[2] It is, however, important to realize that mere superimposition is not a valid criterion of a true hypothesis—the fact that from a new hypothesis is deducible all that is deducible from an accepted hypothesis, and more besides, does not indicate that the new hypothesis is of any value unless that "more" is verifiable. Otherwise such a superimposing hypothesis would violate our criterion of the non-verification of otiose elements in a hypothesis. Superimposition is a valuable criterion, but only when it is combined with a wide range of verified deductions. Not superimposition, but wide verification, is the ultimate criterion.

The other element of the law of parsimony, the principle of simplicity (which is substantially that stated on the preceding page as the usual interpretation of the law of parsimony) is misleading and logically false. If we were making statements which had only a subjective reference, there might be no objection to the statement that the simplest view is the best; but our hypotheses have an objective reference; they pretend to describe objective reality; therefore the law of parsimony implies not only that the simplest statement of reality is the best, but that reality itself is simple. Scientists from Copernicus to Newton believed that nature acts in the simplest way;[3] as long as this belief persisted, the law of parsimony was a natural deduction from it; but today it is difficult to continue to believe that nature is simple.

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[1] *System of Logic*, Bk. III, chap. iv, sec. 1.

[2] Cf. chap. VII, § 13.

[3] I. Newton, *Principia*, Bk. III, "Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes." Quoted in C. H. Patterson, *Problems of Logic*, p. 108.

In many ways, the progress of science has shown that nature is not simple, and that it is by no means true that the simplest hypothesis always explains the observed phenomena. Certainly the four (or six) elements of Empedocles represent a much simpler hypothesis of the chemical constitution of matter than the present view that there are ninety-two elements, especially when we remember that many of these elements are supposed to exist in several isotropes! The Democritean or Daltonian homogeneous atom, with its valency hooks, is much simpler than the Bohr atom, with its galaxy of rotating electrons and highly complex constitution. The economic individual of the nineteenth-century English economists was a much simpler person than the sort of individual contemplated by contemporary economists, and it was precisely the simplicity of their hypothesis that led these older economists to make so many mistakes. The progress of modern astronomy has increased, not decreased, the number of motions attributed to each planet; to Kepler the earth had only two motions, revolution on its axis and revolution around the sun; today the earth is found to have more than a dozen motions! Whitehead and Russell, in attempting to make an analysis of the foundations of mathematics, found that, in the attempt to effect the greatest possible analysis of the ideas and processes of mathematical logic and the solution of the paradoxes of that logic, they were "often compelled to adopt methods, definitions, and notations which are more complicated or more difficult than they would be if we had the second object alone in view."[1] Galileo at first supposed that the velocity of falling bodies is proportional to the space from the point where the motion began; this law is quite as simple in its enunciation as the true law, which is that velocity is proportional to the time.[1]

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[1] *Principia Mathematica*, I, 1.

Perhaps the most notable instance in which simplicity led science astray is the ancient view that the movements of celestial bodies must be circular and uniform. Certainly the most simple and definite supposition is that motion is circular and uniform; but the mistakes of ancient astronomy followed chiefly from attempting to cling pertinaciously to that view. Inequable and non-circular movement was thought to mean disorderliness among divine and eternal things, i.e., complexity, which was intolerable, and so the famous problem was formulated, how to represent the phenomena by equable and circular motions.[2] The love of simplicity and symmetry has as often produced and perpetuated error as truth.

The law of parsimony has been rendered plausible because of the success with which the early modern scientists used it. Copernicus had no other recommendation for his hypothesis except that it was simpler than the Ptolemaic view. He could not deduce any additional phenomena. Kepler was motivated to conduct the long and arduous search for his three laws by the Pythagorean belief that nature is made of numbers and is simple. Galileo delighted in putting his mechanical problems into mathematical form and demonstrating his conclusions by simple geometrical means, often using experiment only for the purpose of making his mathematical demonstration more persuasive. These great founders of modern physics and astronomy were of necessity solving the simplest problems of their science; they had not the technique or knowledge whereby to solve any very compex or difficult problems; and so of necessity the solutions of simple problems were simple. But later scientists have met with much more complex problems, and have found that complex answers are necessary. Galileo deduced from his law of falling bodies that a cannon-ball moves in a parabola; but experiment has shown that Galileo was mistaken, for the friction of the air makes the actual path of a cannon-ball nearer a semicircle than a parabola! The actual path of a cannon-ball was a problem not solvable by Galileo's simple methods, so he oversimplified it and was misled. Today we realize that the law of parsimony is a very limited universal; it applies only to simple problems, and not to the problems which scientists are attempting to solve today.

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[1] Whewell, *History of the Inductive Sciences*, I, 326.

[2] *Ibid*., I, 142 f.

Furthermore, the law of parsimony is not necessary to science. It has been thought that the law of parsimony is needed to eliminate such hypotheses as the assertion that spirits are *verae causae*. We have seen that there are logical reasons for excluding these sorts of hypotheses, quite apart from the law of parsimony.[1] The hypothesis of spirits is excluded as unverifiable because it does not provide for the exact deduction of any verifiable consequences; that is, such a hypothesis is so vague and inexact that no accurate deduction can be made. Anything might be consistent with the action of spirits; hence nothing in particular is uniquely consistent with their presence. We have also seen that no otiose element of a hypothesis may be considered as verified; if the telepathic action of minds can show no observed phenomenon which is not also deducible from the hypothesis of non-telepathic action of those minds, then we cannot accept the otiose element in the hypothesis, that of telepathy, as verified. The reluctance of scientists to accept telepathy, in spite of so many apparent proofs of it, is also largely due to the fact that it is only an empirical law—its explanation is so uncertain—the mechanism of thought transference is not known, so that no rational hypothesis can plausibly be formed. Telepathy, even if it were a proved fact, would be at best only an empirical fact which shows no sign of becoming rational.

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[1] Cf. chap. vii, § 12.

It has been said that only the law of parsimony prevents science from accepting the mythological interpretation of the movements of heavenly bodies, that without it we should still be believing in the daily movement of the god Helios in his chariot across the sky.[1] But the principle of the non-verification of otiose elements in a hypothesis will show us that the only element of our mythological hypothesis which can pretend to be verified is that of the movement of some object across the sky; and when we come to develop the implications of the view that it is a *god* who is moving across the sky into the consequent possibility of influencing his actions by prayer and sacrifice, we see that such a hypothesis can be definitely disproved, unless the term "god" be taken as pragmatically equivalent to "material object."

The Copernican hypothesis did not win its way because of its simplicity only, except with a few mathematicians. Kepler and a few other mathematicians did accept Copernicus' view because of its simplicity, but they had been trained in the discipline of mathematics in which simplicity and elegance are considered a virtue. The Copernican hypothesis first began to gain assent in non-mathematical circles, when Galileo showed that there is more than one center of motion in the universe, that Jupiter's moons revolve around him—the Copernican hypothesis was able to assimilate that fact without material change, whereas the Ptolemaic hypothesis had to be changed radically in its fundamental assumptions (that the earth is the only center of motion), i.e., in its character as a *rational* hypothesis, upon the discovery of this and every other new phenomenon. The wideness of verification of the one hypothesis and the *ad hoc* character of the other convinced the scientific world of the truth of the Copernican view.

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[1] Holt, *The Freudian Wish*, pp. 83 f.

It has also been asserted that only the law of parsimony prevents us from interpreting such a phenomenon as the bending of an iron bar at the same place under repeated stresses, as a case of psychological learning; unless we assume such a form of the law of parsimony as Lloyd Morgan's canon,[1] we have no criterion for choosing between the two hypotheses of molecular disturbance and of psychological learning. Perhaps the greatest difficulty in this case is that psychological learning is as yet only an empirical law; its neural interpretation is so uncertain. Consequently, the hypothesis of psychological learning is so vague that it is not at once apparent that its application to an iron bar is overstraining the meaning of the term. If by psychological learning is meant merely that an event which has once occurred is more likely to occur again in the same fashion, we may well admit the bending of the iron bar as a case, but the rising of the sun would also be a case of psychological learning; it would be a purely empirical generalization. If, however, any rational interpretation is given of that occurrence, so that psychological learning comes to mean something different from a tendency to repetition, two things will become apparent upon analysis: first, that when the hypothesis of psychological learning is applied to the behavior of an iron bar, its full meaning will be more than enough to explain that behavior, and the criterion of the non-verification of otiose elements in a hypothesis will apply; and, second, this hypothesis may enable the deduction of other phenomena in addition to, or contradictory to, those observed, i.e., that the object should (at least usually) be benefited by its learning, or that the object should present a flexible and pliable adaptation to its surroundings. In most cases of this sort, the principle of the non-verification of otiose elements in a hypothesis will bring about the same results as those for which the principle of parsimony is called in. That principle does not involve the objectionable metaphysical and methodological implications of the principle of parsimony. Any other case in which it may likewise be thought necessary to use the law of parsimony will also be found to be dealt with adequately by one or the other of the elements in our general criterion for the verification of a hypothesis. The law of parsimony is unnecessary as well as misleading.

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[1] Stated as follows: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale." Cf. C. R. Griffith, *General Introduction to Psychology*, p. 108.

This law, in its demand for a simple statement, is a criterion that was taken over from the field of deduction—especially of mathematics, where it is useful—and wrongly applied to the field of induction. The simplest mathematical demonstration is decidedly the best—the best to remember and the easiest to apply. Mathematics rightly requires elegance wherever possible, and the mathematician prides himself on the elegance of his work. As a heuristic principle, the simplest hypothesis is also the best to begin with, because its consequences are most easily deduced, and the disproof of the hypothesis may indicate where changes in the hypothesis ought to be made. But such simplicity is no criterion of truth; in the present condition of science, simplicity seems to be rather an indication of the falsity than of the truth of a hypothesis. Simplicity is an artistic and purely deductive criterion, which has done much damage to logic and science, because it was used outside of its proper field. In its place should be put the criterion of the non-verifiability of an otiose part of a hypothesis.

§ 3. The distinction between rational and empirical induction is a very important one. Perhaps the chief cause for the failure of modern logic to deal successfully with the problem of induction has been its failure to make this distinction.

The empirical law merely states the universal occurrence of some uniformity; the rational law is capable of wide deduction and verification in a number of different fields. The empirical generalization is a hypothesis from which is deducible only one type of event—the event stated in the hypothesis. Consequently, the only proof of an empirical generalization is that it has invariably occurred—its proof is by the process of enumeration of instances only.

The proposition that is commonly used as a major premise of deduction in the logic books, "All men are mortal," is a typical empirical law. The only verifiable events which can be deduced from such a statement are that Socrates, Plato, Aristotle, etc., have died and that any other individuals who may be called "men" will also die. This generalization does not enable the deduction of different sorts of verifiable consequences. It is a statement of a uniformity and nothing more. It is quite correct to say, as logicians have been saying, that such a proposition can never be proved to be certain. Not only are there the cases of Enoch and Elijah who were supposed to have been translated without dying, but it is impossible to prove that all men of the immediately past generation have died—every year some persons disappear and no one is ever able to discover what became of them. The legend of the Wandering Jew and other similar myths are based on the impossibility of proving the proposition that "All men are mortal." And yet, in spite of the impossibility of its proof in our present state of knowledge, it does form a valuable and respectable piece of information, which, if not absolutely certain, has been confirmed in a sufficient number of cases, especially of those who have prophesied that they would not die, and which is free from negative instances to such an extent that it is widely accepted as a "fact." An empirical proposition may command respect, even though it cannot be proved true.

Another example, in which science has already emerged from the empirical stage, may illustrate better the difference between the empirical and rational law. For the ancient world the proposition that "one day will be followed by another" or that "the sun will rise tomorrow" was a purely empirical law, and, as such, not fully substantiated. Men could not be sure that tomorrow will invariably come, so they accepted prognostications of cataclysms, in which the sun would fall, the stars turn to blood, etc.—a belief which could never have arisen had the law that one day must follow another been proved invariably true. For many people today, this law is still an empirical law—they have nothing more upon which to base their acceptance of it than the recurring experience of day after day, and the absence of negative instances.

But, for the astronomer, the proposition that day follows day is far from being a mere empirical law. He recognizes it as a valid deduction from the vast hypothesis which constitutes the system of physical and natural law—the laws of motion, the revolution of the earth, the constitution of the astronomical universe. The solar system and its laws are of such a nature that it is necessary that day and night follow each other. A supposed cessation of that regularity would involve the destruction of all our physical and astronomical laws. Those laws have been tested widely; they are based on such a large number of diverse facts of observation and experiment that there is no longer any doubt about their truth. The empirical law that "one day will be followed by another" has become a *deduction* from a rational generalization. That generalization has become so vast a system that the scientist can calculate how great a force would be necessary to stop the spinning of the earth on its axis, and what the consequences of such a sudden stopping would be. He even recognizes that the time will inevitably come when the friction of the tides will slow down the spinning of the earth, and, given a few more data, he could calculate exactly how rapidly that slowing down will proceed, and when the succession of day and night must cease.

But, for the immediately future millenniums, barring a collision with a wandering mass of matter, the scientist is perfectly sure that day and night must continue. He is not sure that such a collision may not occur—for the existence or nonexistence of such masses of matter is still a matter of empirical fact; the distribution of matter in the galaxy of which our sun is a part is not yet rationally understood—so that there still remains a modicum of empiricism in the prediction of the succession of day and night; but, except for this as yet empirical element, the prediction that tomorrow and many tomorrows will come has become a rationally and securely established fact. It is noteworthy that, together with the understanding of the nature of the astronomical universe, the belief in cataclysms has diminished among intelligent people. It is entirely misleading to say that we cannot prove that tomorrow will come; to anyone who understands physics and astronomy, that proof is quite feasible and indubitable, for the proposition that tomorrow will come has been subsumed in a rational hypothesis; and at the same time that that change took place in its logical character, the limitations of that proposition have also become evident.

Another example of the way in which science proceeds from the empirical to the rational generalization is to be found in the prediction of eclipses. The Babylonians predicted eclipses with tolerable accuracy through the discovery, which came as the result of keeping careful astronomical records, of the Saros, a cycle of 223 lunations, or 18 years 11 days, during which eclipses of the sun and moon and other lunar phenomena occur at definite times, so that, from having observed the time in one Saros at which eclipses occurred, they could be predicted for other cycles. Thales probably made in this manner his famous prediction of the solar eclipse that occurred so dramatically in the middle of a battle. This cycle is a purely empirical generalization (a uniformity)—it enables the deduction of nothing more than a regularity of eclipses and of other phenomena which have been observed. When the eclipse was not visible in Babylonia, the priests could not explain that fact correctly; they could merely record it and thank the gods that they had been spared. Their prediction was only approximate; sometimes the eclipse (or a full moon) would occur earlier or later than predicted—which was interpeted as "unnatural" and taken to signify that some "unnatural" event would occur.

Contrast this empirical generalization with the present-day method of calculating eclipses. The Pythagoreans gave the correct explanation of eclipses; today we have determined the rate of revolution of the moon and the earth and the magnitude of disturbing attractions, and can calculate just when these bodies will have arrived at the right position to throw the shadow of the moon on the earth or vice versa, so that we can determine where the eclipse will be visible, when it will commence, and when it will end, to within a tenth of a second. Today, eclipses are no longer predicted as the result of the observation of a uniformity, but are deduced from a vastly larger (rational) generalization, which enables not only the prediction of eclipses, but a great variety of other phenomena too, including predictions of missed eclipses; and one significant result of the establishment of this rational generalization has been a marvelous improvement in the precision and reliability of the prediction itself.

It is an interesting speculation to ask how such an empirical law as the proposition that "all men are mortal" could become a rational law. In its present empirical form, it could never become rational. But if physiological chemists should be able at some future time to show that life consists in a certain condition of delicate chemical instability, and that this particular chemical state carries within itself, as an inevitable element of its own law, the condition that the forces of katabolism will finally prevail over the forces of anabolism, and if this hypothesis were established by a wide range of verification (a condition which might be possible for this hypothesis, whereas it is impossible for the mere statement that "all men are mortal"), then this hypothesis could be established as a rationally induced law, and the further proposition that "all men are mortal" could be deduced from it and established as true beyond a doubt. Needless to say, biological science is far from having established such a hypothesis in its present condition. Living creatures present such highly complex situations that biological science is still largely in the empirical stage. An empirical generalization can never itself become a rational law —it can only become rationally established by being shown to be a deduction from a wider rational generalization.

Mill has given an extraordinarily acute description of empirical laws, which, if understood apart from the rest of his logical theory, gives us a correct conception of them:

> Scientific enquirers give the name of empirical laws to those uniformities which observation or experiment has shown to exist, but on which they hesitate to rely in cases varying much from those which have been actually observed, for want of seeing any reason *why* such a law should exist. It is implied, therefore, in the notion of an empirical law, that it is not an ultimate [i.e., rational] law; that, if true at all, its truth is capable of being, and required to be, accounted for. It is a derivative law, the derivation of which is not yet known. To state the explanation, the *why*, of the empirical law, would be to state the laws from which it is derived. . . . . And if we knew these, we should also know what are its limits; under what conditions it would cease to be fulfilled. . . . . The ascertainment of the empirical laws of phenomena often precedes by a long interval the explanation of those laws by the Deductive Method [i.e.,

the method of rational induction]; and the verification of a deduction usually consists in the comparison of its results with empirical laws previously ascertained.[1]

It is interesting to notice that the formal methods of induction mentioned in the logic textbooks, such as the method of enumeration or Mill's canons, are pre-eminently used in connection with empirical induction. Whenever these methods are used alone to verify an induction, empirical induction is almost always being used; although, when science progresses and rational induction becomes possible, these methods may become the means of deriving some of the deductions from a rational hypothesis which are necessary for its verification. Enumeration and Mill's canons thus have their greatest importance in connection with empirical induction. The tragedy of the theory of induction has been that it has fastened upon the process of empirical induction because it is the one used in the earlier stages of science, and, without seeking for proof, supposed that this is the only method of induction.

Pasteur's famous vaccination of twenty-four sheep, a goat, and several cattle, with anthrax vaccine at the farm of Pouilly-le-Fort, and later inoculation of them and an equal number of unvaccinated animals with anthrax bacilli, was a use of the joint method of agreement and difference.[2] In strict logic, the reasoning was as follows, Hypothesis: This vaccine will immunize against anthrax; therefore in cases in which this vaccine has been used, inoculation with deadly doses of anthrax bacilli will not cause the disease; in order to show that the particular bacilli used in the inoculation were deadly, it was necessary that other healthy animals should also be inoculated. Hence the experiment as performed. With such a hypothesis as this of Pasteur's, wideness of range of verifiable deductions is impossible; the only deduction is that vaccine will immunize against anthrax: it is only an empirical hypothesis; we know now that Pasteur's complete success on that famous occasion was but a happy accident, since, on numerous later occasions, the same vaccine failed to protect and sometimes even caused the disease it was supposed to prevent. We can never be sure of the truth of an empirical law, and must not be surprised when a previously well-established empirical law suddenly breaks down under slightly different conditions.

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[1] *System of Logic*, Bk. III, chap. xvi, sec. 1. (The words in brackets are mine.) Mill goes on to say that an empirical law is characterized by being obtained only by the method of agreement, and must be considered as such until confirmed by the method of difference or accounted for a priori by deduction—a view which contradicts his insight into the nature of the empirical law. None of Mill's canons can give more than empirical results, unless they are used with a rational hypothesis. Deduction we have seen to be incapable of justifying its own premises; Mill had no right to lean upon that—but he saw that, as a matter of fact, in the progress of science, an empirical law is taken up into a system, and so is deduced from more ultimate and rationally established principles. Cf. the next section of this chapter.

[2] Cf. Paul de Kruif, *Microbe Hunters*, pp. 159 ff.

The logical characteristic of an empirical law is, then, that in its proof it is not able to obtain a wide range of deductions, but merely a repeated uniformity. Since it is founded on uniformity alone, the observation of one failure to conform to the uniformity may overthrow such a law. The consequence is that an empirical generalization can never be certain, and can only be at best probable. All the objections that logicians have made against inductive proof consequently hold true of the empirical law—and of that alone.

Mill has asked the question:

> Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing a universal proposition? Whoever can answer this question . . . . has solved the problem of induction.[1]

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[1] *Logic*, Bk. III, chap. iii, sec. 3 fin. Quoted with approval by Ward, *Naturalism and Agnosticism*, II, 226.

The answer to this question is now apparent. The logical methods of proof used in the two cases are entirely different. In the first case, in which one instance is sufficient to establish a law, the method of rational induction has been used, and attention is directed to a "crucial instance,"[1] whereas in the case in which many instances without exception fail to prove a universal law (Mill gives the case of the blackness of crows) only empirical induction can be used, and it cannot be shown that the observed uniformity results from any rational law—it is quite possible that the uniformity is the result of accidental circumstances.[2]

The justification of an empirical law does not consist so much in the number of times the uniformity has been observed to occur, as in the varying conditions under which the uniformity has been found to hold. The more varied the conditions and the larger the number of different fields in which a uniformity has been observed, the more reliable it is. If we should flip a coin and find that it came down heads up for ten times in succession (something which happened four times in Jevons' series of 20,000 throws[1]), we should have no reason to infer the existence of an empirical law that a fair coin, when thrown, will come down with heads up. Yet if we eat spoiled meat once or twice and get sick afterwards, we are sure of the empirical law that spoiled meat brings sickness. In the one case, ten uniform occurrences do not give us what is considered sufficient evidence for the statement of a law, but in the other, one or two cases are sufficient! The difference is due to the rational hypothesis (verified or unverified) by which those occurrences are explained (from which they are deduced). In the one case it is chance, in the other it is a supposed biological connection. If a uniformity appears to be accidental, no number of occurrences, no matter how large, will make us feel willing to assert an empirical law; if it appears to be rational, one case is sufficient. In either case, the absolute number of times an event has uniformly occurred has no evidential value for the truth of the empirical law.

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[1] Cf. chap. vii, § 8.

[2] The instance Mill gives of complete induction by one instance is illuminating as showing that even natural scientists are sometimes mistaken in trusting empirical induction. He uses as an example the supposed law that one determination of the characteristics of a chemical element is sufficient for complete induction (an empirical law). Such a single determination would be sufficient only if it could be proved that each chemical element is the same wherever found. This law had not been rationally established in the time of Mill, but atomic theory had been somewhat established, and sufficient empirical evidence of the uniformity of the elements had accumulated to make that law a common assumption among physicists and chemists. We know now that this assumption was sufficiently close to the truth to serve as a good working hypothesis and was of great service in the development of chemistry; but the discovery of chemical isotropes has made it false, so that we now know that one determination of the atomic weight of an element may be misleading! Not until some rational theory as to the exact constitution and arrangement of the protons and electrons in the nucleus of chemical atoms has been verified shall we be able to deduce with confidence the law of the uniformity of chemical atoms, and to determine the limits of that law.

The reason that we think more highly of an empirical generalization which has a large number of cases to its credit is that we suspect it to be the consequence of some unknown rational law and value it accordingly; a large number of cases has value only because it decreases the possibility that the uniformity is due to mere chance, and makes it more possible that the uniformity is the result of an (unknown) rational law which can be widely verified. It is misleading to assert that the probability of an empirical law increases with the absolute number of cases in which that uniformity has been observed, and to calculate the probability of all crows being black or of the sun's rising tomorrow! The postulates on which the mathematical proof of such a statement is based are erroneous. If I had opened a thousand eggs and found every one good, that uniformity would not justify me in asserting that the probability is 1,000 to 1,001 that the next egg would be good, if from another crate; and if I had such an expectation, based on mere uniformity and not upon the skill of the buyer of eggs, or some other consideration different from mere uniformity, my confidence would certainly be greatly misplaced.[1]

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[1] Jevons, *Principles of Science*, p. 208.

A large number of instances is felt to add to the reliability of a generalization, because we thereby suspect (or know) that the generalization holds under varying circumstances and is the consequence of a rational law. In other words, we believe that our instances are not all the same, that they are not mere repetitions of a uniformity, but are different. It is the variety of the circumstances under which the generalization has been shown to hold that makes it reliable, not the number of instances, because that variety assists in (or enables) the establishment of a rational law from which the uniformity is deducible. A large number of instances precisely alike would not add to the value of the generalization, for the next *different* instance might spoil it; whereas a small number of different instances secured under different circumstances has great evidential value. Professional samplers know that a large number of samples from the same place in a car of wheat has no evidential value as to the nature of the wheat, whereas a small number of samples from *different* parts of the car has almost complete evidential value. Unless the instances are known or are suspected to add to the number of different circumstances under which the generalization is found to hold, they are valueless. It is the *range*, and not the number of instances, that establishes the (possible) correctness of an empirical law.[1] The value of an empirical law depends on the variety of circumstances under which it has been found to hold.

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[1] Uniformity would enable me to make an assertion about eggs from the same crate (after a "fair sampling") or the same shipment, or bought by the same buyer, because I might be able to establish a rational hypothesis from which the goodness of the eggs could be *deduced;* but uniformity would not justify me in making any assertion about the next egg in general.

As to the certainty of an empirical generalization, not much can be known when it is still only empirically established. Some empirical laws, after a long series of uniformities, have been later discovered to be false—for example, Aristotle's statement that "all swans are white," by the discovery of Australian swans; or the view that "all blackberries are black," by Luther Burbank's production of a white blackberry. Other empirical laws have later been found to be certain and invariable—such were Boyle's law of the pressure of gases and Galileo's law of falling bodies (which later became rationally established by Newton).

During the period in which a law is merely established as a uniformity, it cannot be considered as certain. Its degree of trustworthiness depends upon the degree to which all possible circumstances have been investigated to see whether it holds true, or (what is the same thing) the degree to which a comprehensive search for negative instances has been made. It might be said that the trustworthiness of an invariably observed uniformity, an empirical generalization, may be expressed by a fraction whose numerator is the number of different circumstances that have been investigated and under which it has been found to hold true, and the denominator is the total number of circumstances under which an instance of this law could possibly occur—but all possible circumstances under which an empirical law might apply cannot be known until it is rationally established! I know of no other way of definitely expressing the degree of trustworthiness of an empirical law. The statement of a probability (as every other mathematical statement) implies knowledge; with the arrival of knowledge, the probability of an empirical law is changed into certainty. An empirical law can be known to hold only in an indefinite proportion of cases.

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[1] J. M. Keynes (*A Treatise on Probability*, Part III) fundamentally agrees in this matter; he shows that unless we can know a priori that our generalization has at least a small finite probability (so that we can say a priori that if our generalization is false, the probability of a large number of instances is smaller than the a priori probability of the generalization) the number of instances does not add to the probability of a generalization. In other words, the number of instances is only of evidential value in adding to the positive or negative analogy. But Mr. Keynes, like Hume, deals only with the empirical law, and neglects the rational inductive law. A truer statement would be that the number of instances is only of value when it aids in the substantiation of a rational law. Bosanquet (*Implication*, p. 66) takes the same view as Mr. Keynes.

A much better guess as to the reliability of an empirical generalization can be derived from an estimation of the nature of the rational law of which it is likely to be a consequence. It is in this fashion that practical men usually judge the worth of an empirical uniformity. For this reason scientists are rarely content with a mere statement of a uniformity (an empirical law); they try, whenever possible, to explain it—to state the rational law under which the empirical generalization can be subsumed, even though such a rational law cannot, in the present state of science, be demonstrated true. Many so-called "working hypotheses" are of this nature. The truthworthiness and limits of the empirical generalization can then be judged roughly from a consideration of this (assumed) rational law.

In spite of the unavoidable inability of being quite sure of the correctness of an empirical law, such empirically established laws are not to be despised. The empirical law is the form of generalization with which a science begins, and which is only very slowly superseded. As Mill remarks, a rational law is often verified because it explains (permits the deduction of) observed empirical uniformities. The kinetic theory of gases was verified because it explained the empirically established laws of Boyle and Charles, as well as many other phenomena of gases. Such empirical laws are valuable in the early stages of a science, because they are all the generalizations that are had; but science always strives to subsume them under more general and rationally established laws. The sciences connected with medicine, because of the complex nature of their subject matter, are still almost entirely empirical—certain medicines have been found to help in certain diseases, though *why* is not understood—yet who would discard our medical knowledge just because it is to such a large extent empirical? We do not refuse to use what medical knowledge there is, even though the best methods fail inexplicably in a small proportion of cases. Even the best developed of our natural sciences is still compelled to rely largely upon empirical laws—we are still living in the early stages of science. Empirical laws are precious, because even they are so difficult to acquire and so powerful in their application to human life, even if they are not quite sure; and human beings are usually willing to take the risk connected with their use.

The difficulty in completely establishing the truth of an empirical generalization is not that we lack knowledge of all the conditions under which our cases occur, but that we do not know which conditions are relevant and which are not, i.e., which affect the continued truth of the law and which do not. Such knowledge does not come until we know the rational law of which an empirical law is a deduction. These conditions may, however, be guessed at as a result of a partial verification of that rational law. Without a rational generalization, such a statement of the conditions is impossible.

It might be possible to make a complete or nearly complete statement of the conditions under which an observed phenomenon has occurred; but such a complete statement must include that particular space and time, and so, in Aristotle's terminology, it would be a particular and not a universal. We could merely say that under conditions x, y, z, space and time a, b, c, t, event A was observed—our exact statement would say nothing of the future. If we eliminated some of those conditions, such as the space and time, and said that when we have only x, y, z event A will follow, we would be making a venture for which we could have no *logical* justification. It is only because we believe that there is a rational law of which this and similar events are cases that we have any justification for eliminating from our statement any of the conditions under which we observed the event—but this elimination is founded on guesswork, not proof. The fact that many empirical laws have later been found to be invariably true by being subsumed under a rational law justifies science in the venture of faith that is involved in the statement of every empirical law.

While the characteristic of the empirical law is that it is a mere statement of a uniformity, the rational law, by contrast, creates a system. The rational law enables the deduction of *varied* consequences, and these varied consequences are taken up and united into one concept by the fact that they are deduced from one (complex) law. This system-making characteristic of a rational law has been noticed by logicians. Nevertheless, it is to be observed that the verification of a rational law does not consist in the fact that it creates a system; it consists in the correspondence of the deductions with the observed facts in a sufficiently different number of fields. The system-making characteristic of a rational law is not the proof of the truth of the law, but the result of its being a rational hypothesis, rather than an empirical one. Both true and false rational hypotheses can form systems; every well-made work of fiction is an illustration of that fact: the fiction is not true because it is concatenated; it is false because some of the items of the system do not correspond to the events of experience. The greatest intrinsic difference between the rational hypothesis and the empirical generalization lies in the system-forming character of the former.

Since natural science was in its infancy in the time of Aristotle, it is not surprising that he should have known nothing of rational inductive proof. His chief interest was in biology, in which classification, a purely empirical process until the time of Darwin, was the chief procedure. Consequently, it is not surprising that Aristotle should have fallen back upon the empirical method of perfect enumeration; for it was the only method available to him. In the time of Hume, who was the chief critic of inductive logic in the modern world, some phases of science had developed rational proofs; but Hume was ignorant or oblivious of the existence of such, and directed his attack solely upon the empirical generalization. He showed correctly that an empirically established law can never become certain, and must at best remain possible only. Hume was not sufficiently interested in science to understand its true nature. Anyone who had a genuine insight into mathematics or science would never have said that geometry is only probable!

Scientists have made and proved their discoveries true, yet their methodology has been developed and spread by imitation of the outstanding discoverers, and corrected by the criticism of fellow-scientists, although they have not been able to state their method explicitly. Logicians, whose business is the analysis of methodology, have rarely had more than an amateur interest in science. William Whewell was an original scientist *and* a logician; he first stated the method of rational inductive proof, though in such obscure form that Mill, who was enamored of his inductive methods, did not understand him; while later logicians have followed Mill rather than Whewell.[1] So logic has confused the rational generalization with the empirical law (probably because their "form" is the same) to the detriment of logic and the confusion of philosophy.

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[1] It was not until the outline of this theory of induction had been fully formed in my mind that I noticed fragments of it mentioned in the logic texts, and, by tracing those notions (in particular, that of hypothesis) back to their source, I discovered Whewell's formulation.

§ 4. In the foregoing statement of the methods of rational and empirical induction, we have left no place for the use of Mill's canons as he stated them. Yet these canons, or something very much like them, are undoubtedly used in scientific proof. It is true that logicians have been doubtful as to their exact significance and have usually denied them any great evidential value. Yet they have remained in the logic texts because they are somehow used. We must then face the problem of the interpretation of these canons. How do they fit in with the theory of rational induction? Careful analysis shows that they are, in reality, commonly used canons of *deduction* from a causal hypothesis.

In working with those types of hypotheses that deal with causes or effects, a particular sort of deduction is very often drawn. If my hypothesis is that x is the cause of y, I can immediately draw the conclusion (by the aid of the definition of "cause") that, wherever x is present, y should also be found; or that of two otherwise similar cases, in one of which x is present, and in the other of which x is absent—in the first, y will be found, and, in the second, y will be absent; so that we may thus tell whether there is a connection between x and y; or that if I take two groups of cases, in the first of which x is always present, while in the second x is always absent, then y should be present in each case of the first group and absent in each case of the second group; or that if it is possible to make x vary in degree, y should vary concomitantly; or that if phenomena can be analyzed into a set of causes and effects, and if all the causes except x are subducted, x and y alone will be left. Thus all of Mill's canons are deductions from the *hypothesis* of a causal relationship. By stating the definition of cause (or effect), these rules can be derived from that definition by the usual methods of deduction. A premise put with one of these rules we have thus derived, stating that, for example, the cooling of moist air is the cause of the deposition of dew, would bring the conclusion that, whenever moist air is cooled, dew should appear. Once that deduction had been drawn from the hypothesis, the experiment of producing the cooling of moist air could be performed and it could be observed whether dew is produced or not, in order to verify the deduction from the original hypothesis. Mill's canons thus generalize very common types of deduction from hypotheses dealing with causal relations. From the viewpoint of formal logic, Mill's canons are inexact formulations of canons for immediate inference (transformation) from a causal hypothesis. That is, by the application of Mill's canons (when correctly stated), a conclusion can be drawn from a causal hypothesis without the use of another premise.

We can now restate Mill's canons and show that they are methods of deduction from causal hypotheses. An illustration of the canon of agreement has been given in the foregoing paragraph. That canon should be stated approximately as follows: In every causal relationship, in all the cases in which the cause is found, the effect will tend to be present, and in all the cases in which the effect is found, the cause may be present. We have stated that the cause only *tends*[1] to produce its effect, because there are many cases in which the effect is prevented from appearing by the presence of some other factor. We have also stated that the effect does *not necessitate* the presence of the cause, but merely indicates the possibility that a particular cause may be present, since the same effect may have several different causes.

As an example of the canon of difference, we take the experiment of the electric bell in the receiver of an air-pump. The hypothesis is that air is the carrier of sound; consequently, by this canon, if we removed the air, sound will not be heard; whereas if the air is present, the sound will tend to be heard. This deduction, when made, may be verified by experiment. The canon may be stated approximately as follows: In every case of a causal relationship, if the cause (or an indispensable part of the cause) is removed, the effect will usually be found to have also been removed; but if, under similar circumstances, the cause is introduced, the effect tends to be present; again, if the effect (or an indispensable part of it) is removed, unless a possible impediment to the action of the cause is introduced, the cause will be found to have been removed; whereas, under similar circumstances, if the effect is introduced, the cause may be found to be present. It is the ease of inference when the effect is removed that makes this canon so useful.

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[1] A phenomenon "tends" to occur when, in the absence of any hindering circumstance, it will occur.

The other canons may be similarly restated; they are formulations of a simple set of sequences which are directly deducible from the definition of a cause or an effect—that where the cause is present, the effect tends to be present; where the cause is absent, the effect tends to be absent; where the effect is present, the cause may be present; and where the effect is absent, the cause may be absent. (Because of the "plurality of causes" and the existence of "non-reciprocating causes," we cannot argue from effect to cause or vice versa with certainty in all cases, a circumstance that Mill noted.) Mill's canons are commonly used methods of deduction from hypotheses, instead of being inductive in their nature. The reason they appear to be inductive is that the inductive sciences frequently deal with causal relations, hence make the sorts of deductions which Mill formulated in his canons; such deduction is an essential and important part of the inductive method, but the use of Mill's canons by the inductive sciences does not show that these canons are inductive in nature.

The proofs that Mill's methods are deductive in their logical nature are many. As Mill formulated his canons, they are clearly inadequate to establish any causal connection. His failure to interpret the type of inference dealing with the relation of cause and effect has been generally recognized by logicians.

The "plurality of causes" and the existence of "non-reciprocating causes" may render any of Mill's canons null and void. The effect *a* may be found without the cause *A* because it happens in a particular case to be produced by another cause *B* instead of by *A*. Again, the cause *A* may fail to produce the effect *a* because of the interference of another cause *C*, which prevents the appearance of *a*. Mill admits that this circumstance vitiates the method of agreement, but fails to see that it also vitiates the canon of difference. It is possible to have (in accordance with the canon of agreement and our convention as to the meaning of letters) two instances of the presence of *a* which have nothing else in common, so that one instance of *a* occurs with *ALMNO*, etc., and the other with *BDEFG*, etc. By the canon of agreement, there would be no cause or effect for *a!* It is, moreover, possible (using the canon of difference) to have two instances, in one of which *a* occurs with *AFGHIJ*, etc., and in the other *a* is absent, with *ACFGHIJ*, etc., present. Since these two instances are similar, *A* cannot be the cause of *a;* in fact, there can be no cause of *a*, since nothing is present only where *a* is present and absent where *a* is absent! To give a concrete example, it is possible to have the germs of tuberculosis present in the lungs and yet not to have the disease of tuberculosis—hence, in accordance with the canon of difference, the germs of tuberculosis do not cause the disease! Since Mill's other canons are only variations of the two fundamental canons of agreement and difference, they are liable to this same inanity. Mill's canons fail utterly in dealing with the "plurality of causes" or with "non-reciprocating causes."

Under these circumstances, only the method of hypothesis and deduction can deal with the situation. We assume that, according to the law of gravitation, all material particles fall toward the center of the earth, but account for the fact that large stones remain on the tops of mountains for centuries (a fact which would overthrow the law, according to Mill's canons) by the supplementary hypothesis that the stone exercises a pressure which may be unobserved because of the intervention of other material particles; consequently our law is stated in terms of *tendencies*—the stone *tends* toward the center of the earth, though it may *never* get there. Mill's canons cannot deal with tendencies, only with actualities; whereas the method of hypothesis easily deals with tendencies. We assert that tuberculosis bacilli produce the disease, and add the supplementary hypothesis that some circumstance in the physical condition of the person observed prevents them from exerting their usual effect. The composition of forces or of causes has become a well-accepted part of the method of deduction from hypotheses, whereas Mill's methods cannot assimilate it.

Such a restatement of Mill's canons as that given by Mr. Joseph, which makes all of them merely methods of elimination, does not give to them their full value; the examples of their use which are given by Mill, such as the investigation of the nature of dew,[1] show that they are as often and more often positive than negative in their use. The apparent cogency of Mr. Joseph's contention is due to the difficulty of fulfilling exactly the conditions laid down by those canons. Because of the difficulty or impossibility of making sure that two instances agree in *only* one circumstance, or that they differ in every circumstance *except one*, it follows that where A and a are co-present in a number of cases, it may yet be true that *a* is caused by an unobserved factor, X. On the other hand (neglecting "non-reciprocating causes," so that a cause invariably produces its effect), if we find a single instance in which A is present but a is absent, it is proved that A is not the cause of a. Since one instance will suffice to disprove a causal connection, whereas (because of the "plurality of causes") several instances are insufficient to prove a causal connection, the method of agreement appears as a method of elimination. But once we admit that a cause does not invariably produce its effect (i.e., that there are "non-reciprocating causes"), the method of agreement becomes as untrustworthy for elimination as for proof![1] Similar reasoning will apply to the other canons. They appear to be methods of elimination because it is easier to apply them in this way; but once admit the existence of "non-reciprocating causes" and all of them become untrustworthy. In reality they can only be safely and usefully applied as methods of deduction from a hypothesis; the apparently fruitful use of those methods is almost solely in that manner. Only the method of induction by the use of hypotheses, with its full wealth of deductive power, is able to deal with concrete cases of induction in their full complexity.

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[1] Mill, *Logic*, Bk. III, chap. ix, sec. 3; or J. Herschel, *Preliminary Discourse on the Study of Natural Philosophy*, Bk. II, chap. vi, secs. 163–69; or Wells, *Theory of Dew*.

In spite of the inability of Mill's canons to deal adequately with inductive inference, they, or something closely resembling them, are so valuable that they are commonly employed in inductive science. Furthermore, the sort of inference represented by Mill's canons is clearly very ancient and universal—cases of inference by what appear to be Mill's canons are found among primitive men or savages—the method of difference seems to supply the conclusion that the African who had broken off a piece of an anchor to make a hoe and died the next day was killed by that anchor, or that the good fortune of the Eskimo who caught a seal shortly after finding and picking up a ham-bone was due to the ham-bone. Herodotus used the methods of agreement and difference in disproving a theory as to the cause of the floods of the Nile.[1] The form of inference represented by Mill's canons is thus extremely valuable; it is positive as well as negative in its nature, and it is so widespread in its use that it antedates any statement of logic. Any interpretation of Mill's canons must account for the fact that these methods are not merely used in the elimination of false hypotheses, but that they aid in the substantiation of true suppositions, although they are not usually by themselves sufficient to assure the truth of any view. Our interpretation of these methods as deductive in nature restores them to their proper place as one of the valuable and important parts of the scientific method of proof. As Mill stated them, they have little positive logical force and at best only a doubtful negative evidential value; as we have stated them, they become a valuable means of positive as well as of negative proof, since they are no longer relied upon to provide the *whole* of the evidence required for the truth of a theory, but merely one of a range of deductions.

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[1] Mr. Joseph has admitted this fact. *Introduction to Logic,* chap. xxii.

Still more important as showing the deductive character of Mill's canons is the fact that they are rarely or never used without a hypothesis. Each canon assumes that we are considering the connection between two phenomena before we apply the canon;[2] this connection between two phenomena is the hypothesis, as a consequence of whose assumption Mill's canon is applied to the case. Mill's canons are never used literally as he stated them. Scientists never proceed by listing *all* of the concomitants of the presence and absence of the phenomenon that they are investigating; if they should attempt to do so, several lifetimes would not exhaust them. Yet exactly such a Baconian listing of circumstances is implied in the symbolic statement of Mill's canons. Without such a listing, we could never be sure that *only one* circumstance was different or in common between two or more instances of the phenomenon under investigation. Where Mill's canons are used, the connection between two phenomena is already suspected, and the invariability of the connection is being tested—the use of Mill's canons is logically subsequent to the formation of a hypothesis. Mill himself admits that while the canon of difference demands that "the two instances which are to be compared with one another must be exactly similar, in all circumstances except the one which we are attempting to investigate," yet "this similarity of circumstances needs not extend to such as are already known to be *immaterial to the result*."[1] Mill thus admits the contention that the similarity of the instances of the canon of difference or the difference of the instances in the canon of agreement need only extend to "relevant facts."[2] Then on what basis is a "fact" to be decided to be relevant? Evidently only on the basis of some hypothesis. Here, again, the use of a hypothesis is fundamental to the use of Mill's canons.

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[1] *History*, ii. 20.

[2] The method of residues is an apparent exception, but in a concrete application of the method, such as the discovery of the planet Neptune, it can be seen that the hypothesis precedes the use of the canon. Cf. R. Ball, *Great Astronomers*, pp. 340–60, or C. H. Patterson, *Problems in Logic*, chap. v, especially p. 83, remark 3.

We might go through various examples of the use of these canons and show that in each case a hypothesis is involved. A striking example is afforded by a common illustration of the method of agreement, Sir David Brewster's discovery of the reason that mother-of-pearl is iridescent. He happened to take impressions of a piece of mother-of-pearl in beeswax and in resin, and was surprised to see the iridescence reproduced upon their surface. That phenomenon suggested to him the hypothesis that it was the form of the surface which caused the colors, and so he proceeded to impress the form of the mother-of-pearl upon other substances. The suggestion to do so is a deduction from his hypothesis, although it might also be interpreted as the use of the canon of agreement. But the final proof of his hypothesis was produced by turning the microscope upon the mother-of-pearl, and perceiving that its surface is composed of tiny ridges and hollows produced by the laminated structure of the pearl. This investigation by the microscope is plainly another deduction from the hypothesis, but how is it to be connected with any of Mill's canons? It does not come under any one of them, yet it is evidently an important part of the inductive proof. Mill's canons can at best explain only a part of induction, whereas the account of induction as proceeding from a hypothesis accounts for even the most complex cases.

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[1] *Logic*, Bk. III, chap. viii, sec. 3, p. 281. My italics.

[2] F. C. S. Schiller, *Formal Logic*, chap. xix, sec. 7.

In the examples that are given by Mill himself, in each case, we can easily perceive the hypothesis that underlies the use of his canons; indeed, he often states it himself. In the first set of examples, Baron Liebig's investigations,[1] the hypothesis from which the investigation proceeds is that death by metallic poisons is due to the conversion of the animal substance into a chemical compound. In the second set, the hypothesis is (as stated by Mill himself) "that the excitement of electricity in any body has for one of its necessary conditions the possibilty of a simultaneous excitement of the opposite electricity in some neighboring body."[2] In the third set, the hypothesis is that the deposition of dew is the result of the object bedewed being colder than the surrounding air; all the various circumstances under which dew is deposited are shown to be cases in which the bedewed object is colder; finally, this hypothesis is shown to be a deduction from the more general laws of aqueous vapor, and exceptions to the hypothesis are shown to be accounted for by other deductions from these more general laws.[1] In the fourth set, the hypothesis is that "the greater the degree of muscular irritability at the time of death, the later the cadaveric rigidity sets in, and the longer it lasts, and the later also putrefaction appears, and the slower it progresses."[2] It is possible in each case of the apparent use of Mill's methods to show that their use is dependent upon some hypothesis, which is usually stated by the investigator before he gives an account of his experiments or observations, in order that the reader may be enabled to deduce the observed phenomena from the hypothesis.

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[1] Mill, *Logic*, Bk. III, chap. ix, sec. 1.

[2] *Ibid.*, sec. 2.

If Mill's methods are ever used without some hypothesis, it is not in the logical process of proving the truth of some proposition, but in the *discovery* of some hypothesis. The existence of residual phenomena (which fact Mill has capitalized into the method of residues; it is to be doubted if that method is ever used with probative value; none of Mill's examples seem to be a literal use of this method) very often furnishes the suggestion for the formation of a new hypothesis. For example, Sir John Herschel suggested that the discrepancy between the calculated and observed return of a comet may be due to the resistance of the medium through which it travels;[3] the difference between the calculated and observed rate of propagation of sound in a liquid suggested to Laplace that this retardation is due to the heat developed in the act of condensation.[4] But the proof of Laplace's hypothesis was not by the canon of residues; it was by taking his suggestion as a hypothesis, developing its consequences by mathematical deduction, and thereby finding a complete explanation of the residual phenomenon—a procedure which is indicated in the passage that Mill quotes from Herschel. Likewise, scientists may search for the unknown cause of

[1] *Ibid.*, sec. 3.
[2] *Ibid.*, sec. 4.
[3] *Ibid.*, sec. 5.
[4] *Ibid.*

poliomyelitis by seeking to discover what phenomena occur concomitantly with the spread of that disease, although such concomitance, by itself, can never establish the causal relationship. Where Mill's canons even approximate a literal use, they are more often methods of research than methods of proof.

The process of the *discovery* of a hypothesis is not logical in its character; while each science can give good advice as to methods that may suggest a hypothesis, there are, however, no rules that guarantee the discovery of any hypothesis. Such a procedure is essentially lawless and hence non-logical. While the examination of the concomitants of a phenomenon by a method that is in rough accordance with Mill's canons may suggest a hypothesis, yet, as logicians, we must refuse to consider such processes, for our only concern is with the proof of a proposition that is a candidate for knowledge, not with its discovery.

The deductive character of Mill's canons is furthermore a consequence of the fact that these canons all assume universal causality, and that, if the law of universal causality is denied to apply to any case, Mill's canons lose their entire force for this case. Mill's methods can all be deduced from the definition of cause or effect; they are only apparently inductive, because they deal with material that is supposed to be inductively known.[1] Mill's canons are therefore canons of deduction, which are commonly employed in inductive reasoning.

§ 5. It remains to state the meaning for logic of some terms commonly employed in inductive reasoning: description, explanation, cause, condition, and possibility.

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[1] Bradley has also concluded that Mill's methods are deductive in their character (*Principles of Logic*, Vol. II, Bk. II, chap. iii, especially sec. 10, p. 337). He shows that they deal only with "universals," i.e., with conceptual statements, and not experience; he shows that the process of exclusion, which he finds fundamental to the use of these methods, is logically similar to the disjunctive syllogism.

By a description, in its strictest sense, is meant a directly verifiable hypothesis or immediate description.[1] The term "fact" is also often used in this sense. A description is an account of my experience as it appears to me, and nothing more. Such a description necessitates a highly sophisticated mind; it is a difficult task to give an immediate description instead of an interpretation of my experience. The rareness of strict description is shown by the difficulty or impossibility of finding the words to express experiences exactly, in all except emotional fields of experience. I see what appears to be a trapezoidal surface with what are apparently vertical three-sided sticks under it, and I speak of a rectangular table with square legs. Words for giving an exact description of what I see in such a case are lacking. The reason is clear: it is of no importance for practical life to describe exactly the different appearances of the table, and consequently the table actually appears rectangular to me, although I do not see it as rectangular. Often it is only the painter who sees things exactly as they appear, because their exact appearance is of importance only to him. Most terms express a synthesis of experience, as steam-shovel or college; the reason is that, for practical life, it is the complex synthesis to which I react; the separate elements of the experience have little or no significance by themselves; habit has induced me to make unconscious syntheses of the various elements of experience before they come to consciousness. For some professions, such as painting, the separate elements of some kinds of experience have importance, and consequently the habit of observing them is developed; the painter sees blue shadows where the layman perceives them as black.

The consequence is that descriptions, in the strict sense, are very rare. More often description is an account of experience with a minimum of hypothetical construction, or with only those hypothetical constructions that will be accepted without question by my audience. I may talk about rectangular tables or doors, or about solid blocks of wood, quite recognizing that I did not see them as rectangular or solid, and that my audience may also recognize that fact; but, because they will accept a description in those terms without misunderstanding, I use such terms. A description is, then, a more-or-less close approximation to a directly verifiable hypothesis.

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[1] Cf. chap. vii, § 4.

§ 6. A phenomenon is explained when it is deduced from a verified hypothesis, and the explanation consists in that deduction itself. While, of course, the word "explanation" is used in other meanings, sometimes with the meaning given to "description" in the foregoing section, yet in science, an explanation nearly always is a deduction from some hypothesis. When we "explain" heat as motion, we mean that the experience which we call "heat" can be deduced from the hypothesis of motion. We mean that the motion of the molecules of the red-hot poker causes a motion of the molecules of the water into which I thrust it; that causes certain of them to move rapidly, to escape from the surface of the liquid and become a gas; then these molecules, upon coming into the cooler air, condense and appear as the vapor which I see. So one after another of the phenomena which are connected with the experience I call heat may be deduced (with the aid of other natural laws) from the hypothesis that the particles of the "hot" object are in motion. A speck in the sky is "explained" as a planet when it is possible to take the hypothesis of there being a planet located at a certain distance from the earth moving with a certain velocity in accordance with the laws of gravitation and deduce from that hypothesis the appearances of the speck in the sky.

There are, consequently, as many kinds of explanation as there are kinds of hypotheses—verbal explanation, which is deduction from some tautologous word (e.g., water makes things wet because it has a humidifying property—a deduction from the comparatively unfamiliar and hence satisfying word "humidifying" to the word "wet"), empirical explanation, which is deduction from an empirically established hypothesis, and rational explanation, which is deduction from a rationally established hypothesis.

§ 7. The Humean conception of a cause is that a cause is an invariable antecedent.[1] That conception would make day the cause of night and night of day—whereas for astronomy neither is the cause of the other; both are considered to be caused by the rotation of the earth. A cause is not invariably followed by its effect—pulling the trigger of a gun is the cause of the explosion, yet if the powder is wet, the explosion does not occur. Again, the effect may be produced without the cause: the explosion of the cartridge may be caused by hitting the hammer of the revolver without pulling the trigger.

What, then, is a cause? It is not an invariable antecedent, but a *possible* antecedent that is conjoined to its effect by a satisfactory hypothesis. A cause is conceived to contribute directly or indirectly to the production of its effect in accordance with some verified hypothesis. Only a deductive system can take care of the complexities to be found in causal relationships, where causes are prevented from producing effects, and effects are produced by various causes. The proof of a process of causation is an inductive procedure—it is the verification of a "causal" hypothesis. Pulling the trigger is held to cause the explosion of the powder because that connection has been established as an (empirical or rational) hypothesis. That statement does not mean that each time the trigger is pulled the explosion will occur—a huntsman does not expect such a miracle—but that, as the result of induction, a causal relationship has been established between those two events.

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[1] "A cause may be defined to be the object or event which immediately precedes any change, and which existing again in similar circumstances will be always immediately followed by a similar change." Brown, *Essay on Causation*, quoted in Jevons, *Principles of Science*, p. 224. But on what Humean basis can we *predict* the effect from the cause?

Causality is not, therefore, a matter of experience,[1] for no indirectly verified hypothesis is a matter of experience; it is superadded to experience. A causal explanation (by which is meant deduction from a hypothesis which asserts a causal sequence) does then transcend the experience which it explains.[2]

The four Aristotelian causes may be assimilated to this account of causality by a slight change in their meaning; they then become equally real, though their importance varies in different fields. The ancient conception of a final cause, the hypothesis of an anthropomorphic power in inanimate substance, which pulls an event into action, has been given up by modern science as unverified and otiose. In that sense the concept of final cause is no longer used. Its place is taken by the human purpose, which seeks to bring about a future result. Psychology and the social and political sciences are continually utilizing hypotheses that contain or imply purposes, motives, impulses, the libido, or the like—these may well be called final causes. The formal cause, in the modern sense of the result that is in mind, is also one of the factors in a modern psychological account of causation. The material cause, conceived as the chemical or physical characteristics of matter, and the efficient cause, the kinetic force, have become amalgamated in modern physics. Modern science, with its highly complex account of physical and psychical causation, includes all of the four Aristotelian causes (giving to each of them its own interpretation), and, by contrast, the Aristotelian account of causation appears highly simple. Modern science notes, not four separate and distinguishable causes, but endless and highly complex chains of causes co-operating to produce any single effect, so that it is conceivable that in order to explain completely (i.e., give a deduction from rational hypotheses *only* of) any single event, nothing less than the whole universe would be needed.

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[1] F. C. S. Schiller, *Formal Logic*, pp. 287 f.

[2] The problem of the possibility of the transcendence of experience will be discussed in chap. xiv, and the reader is asked to keep his mind open until he comes to that discussion.

§ 8. Sometimes we speak of *the* cause of an event, as distinct from its conditions. We thus pick out one of the causes (in the sense of the word "cause" in the preceeding section) and say that it is *the* cause, while the other causes are called conditions. A condition would then be any prerequisite of an event, which is seen to be such in accordance with some verified hypothesis—what we have called a "cause" in the preceding section. Any event needs the co-operation of a number of causes, and this circumstance is expressed in the statement that they are conditions; it is popularly supposed that an event can have only one cause; since scientific analysis has shown that many are necessary, they are called conditions.

The cause then becomes the one of those conditions prerequisite for the happening of an event that is important for our present purpose. For the chemist, the chemical constitution of gunpowder is *the* cause of the explosion; for the gunman the pulling of the trigger is *the* cause; whereas for the juror or the moralist it is the state of mind of the murderer. Then the term "cause" comes to be relative to human purposes; so the scientist uses the term "condition" and refuses to speak of causes. A curious exception to this situation is to be found in the science of medicine, which still deals largely in "causes"—the bacillus is said to be *the* cause of typhoid fever. Here there is one condition that is practically of more importance than such other factors as the chemical reactions of the body to the presence of bacterial poisons, the state of nourishment of the organism, etc., and so the term "cause" has remained in use. This continued use of the term "cause" is correlated with the still largely empirical condition of medical science, so that the other "contributory causes" are not envisaged as easily as the merely empirical presence of the bacillus. Whether a science happens to use the term "cause" or "condition," the notion underlying it is the same—an inductively established hypothesis.

§ 9. It may be interesting to inquire what, in the light of our present investigation, are the meanings of the ancient categories, possibility and impossibility. With the founding of all knowledge upon an inductive basis, there must be a change in the meaning of these terms.

It is easy to see that impossibility cannot any longer mean inconceivability. Too many inconceivable things have been conceived; the human mind has shown itself to be so flexible, especially in connection with non-Euclidean and other geometries, that we can no longer say that it cannot think certain concepts. It must be remembered that the strongest objection made against Newton's theory of gravitation was that it assumed action at a distance, which was thought to be inconceivable; while today contact has disappeared—in physics there is no longer any action that is not action at a distance, even though that distance be infinitesimally small! When human minds have succeeded in thinking parallel lines that meet at a finite distance, or a whole that is mathematically similar to its parts, it is difficult to say that anything is impossible for them. Literal inconceivability would not be a proof of impossibility, except only in a mental world.

We must likewise drop the time-honored conception of self-contradictoriness as the criterion of impossibility. We have seen, in dealing with the necessary truth,[1] that self-contradiction belongs to a deductive system only, and depends upon that system. Outside of that deductive system, self-contradiction has no meaning. In different deductive systems, the same proposition may have different characteristics. For example, the proposition, "The sum of the angles of a triangle is more than two right angles," is a self-contradictory proposition in the system of Euclidean geometry, whereas it is a self-consistent proposition in hyperbolic geometry; furthermore the proposition, "The sum of the angles of a triangle is equal to two right angles," would be self-contradictory in either of the non-Euclidean geometries (except in the special case that the parameter k becomes infinite). In Euclidean space, a plane figure whose angles summed to more than two right angles could not be a triangle, whereas in non-Euclidean space a plane figure whose angles summed to exactly two right angles could not be a triangle! The view that the self-contradictory is the impossible has been plausible because philosophers have tacitly assumed that there is only one deductive system—the one assumed in ordinary conversation.[1] Careful thought should have cleared away this prejudice.

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[1] Cf. chap. vii, § 14.

What, then, is impossible? If we are seeking a criterion of absolute impossibility, we must declare that nothing is absolutely impossible. There is no criterion of absolute possibility or impossibility. Then these terms have no absolute meaning. We must reject the rationalists' cut-and-dried world of possibilities and seek only the actual.

Yet the terms "possibility" and "impossibility" are continually used; we ought not to allow two such valuable and time-honored concepts to disappear, or leave them to the unscientific for misuse. Hence we may give them a new meaning consistent with our theory and with common (not always with philosophical) usage. In ordinary discourse, any statement that is contradicted by some known truth is called impossible. "It is impossible for stones to fall upward"—not absolutely impossible, not inconceivable, but impossible on this earth with the present set of known laws. Then the false or that which is the contradictory of a true statement is the impossible.

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[1] Kant based his argument for the forms of sensibility upon the assumption that Euclidean space is the only possible sort of space.

What, then, is possibility, not absolute possibility—for, absolutely speaking, anything is possible—but possibility in harmony with our conception of knowledge? If natural causation is universal, then only the actual is possible. But we do not know that the deterministic hypothesis is true; nor do we know all natural laws. Hence we speak of possibility—not in any absolute sense, but because of our ignorance. The merely possible are those statements that are not impossible, i.e., not contradictory to any known truth, yet not sufficiently well substantiated to be known with certainty. Possibility is then relative, a name for human ignorance; when a statement becomes known to be true or false, it drops from the realm of the merely possible to that of the certain. Then, if we cannot prove the truth of Euclidean geometry, plane triangles whose angles sum up to exactly two right angles are merely possible; when Euclidean geometry is definitely established to be the only actual geometry, they will become certain; if non-Euclidean geometry should be proved true universally, they would become impossible. With this meaning, we can continue to speak of the possible and impossible, while not implying a rationalistic absolute deductive system in the use of those terms.

§ 10. We are now ready to sum up our conception of induction in the statement that there is only *one* complete logical process, that of rational induction, while all other logical procedures are, in reality, merely aspects of this fundamental logical procedure. Definition is an essential prerequisite for deduction and immediate description. Deduction of whatever sort, whether Aristotelian, mathematical, or logistic, is the procedure of elaborating the consequences of a hypothesis, for deduction always has to make some fundamental assumptions, and the necessary existence of primitive propositions for every deduction shows the inevitable dependence of deduction upon some hypothesis. The primitive proposition can only be validated by a process of induction.

The inductive procedure of immediate description is likewise one aspect of rational induction. The process of deductive elaboration of the consequences of a hypothesis is wholly conceptual. Deduction proceeds by showing that the truth of one proposition follows from that of another—deduction can never get out of the realm of propositions, mental creations, to non-conceptual experience. The verification of a hypothesis, in the process of rational induction, consists in the comparison of its deduced consequences with experience; that comparison, when most carefully done, is by making an immediate description of an experience, and comparing that immediate description, which is a conceptual creation, with the conceptual results of the deduction from the hypothesis. Thus immediate description is a necessary part of rational induction. When it is used by itself, it betrays its relationship in the fact that no further conclusion can be drawn directly from an immediate description; such an inference can only be made by the help of rational induction.

In many cases, especially in perceptual judgment,[1] we do not bother to make a careful immediate description of our experience; we compare the deductions from our hypothesis directly with our experience. But such a comparison is strictly impossible, for it is a comparison of two incommensurables—a proposition with an experience. Even in immediate description, we do not make such a comparison; we compare the meaning of our description, which is directly experienced, with the experience we have—we are comparing two experiences, not a proposition with an experience. But we easily learn to take short-cuts in actions that are commonly performed, and so seem to be making the impossible comparison between a proposition and an experience. What is really done is to translate the proposition into the experience that it denotes, and then compare two experiences, the one that is denoted and the other that is actually had. Such a translation of propositions is merely the reverse procedure from that of the immediate description, and implies the immediate description. Thus immediate description is one of the steps invariably performed or implied in rational induction.

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[1] Cf. chap. xi, § 2.

The process of empirical induction is likewise only one aspect of rational induction. It is what rational induction becomes when there can be no wideness of deduction, when only one deduction is possible from the hypothesis—the assertion that a certain uniformity invariably occurs. Since the verification of a rational induction depends upon the possibility of many different verifiable deductions it should logically follow that an empirical induction can never become certain. That such is the fact is the unanimous testimony of logicians. Empirical induction is thus an inadequate form of rational induction. Its advantage lies in the fact that rational induction demands a broad generalization and wide knowledge, which is difficult or impossible in the early stages of the study of any subject; whereas empirical induction can be used to give tentative conclusions, whereby numerous experiences may be summed up and made usable until the method of rational induction can be applied.

Thus we see the unity of all logical methods as aspects of the general method of rational induction. It has been the tragedy of philosophy and scientific method that this logical method has been the one which has been the least recognized of all logical methods.

CHAPTER IX

IS CERTAINTY POSSIBLE?

§ 1. We must now take up the problem we shirked at the beginning of chapter vii, and ask ourselves whether certainty or knowledge is ever possible by the method of rational induction. Logicians have generally assumed that certainty can somehow be had; but they have usually been extremely critical of any method for gaining ultimate truth, and so far, no theory has emerged safely from their critical examination. We must discuss the important objections that have been urged against the theory of induction, see wherein their force lies, and consider how far these objections militate against the theory here advanced.

Perhaps the most destructive argument made against the certainty of inductive argument was that of Jevons. He reacted against Mill's assertion that certainty could be had, and adduced many illustrations of seemingly certain truths, which can theoretically be seen to be doubtful. In every case it will be found that he is objecting either to the assumption of the uniformity of nature or to the assumption that certainty can be had by a merely empirical proof (in our sense of the term "empirical"). Let us take his examples and analyze them.

Jevons asks how we can be certain that all water contains one part of hydrogen in nine.[1] Now the analysis of chemical compounds is a purely empirical process. The chemist is so certain that the exact constitution of chemical compounds is not a matter of chance, but due to the characteristics of the chemical elements, that he is quite willing to take the evidence of one quantitative analysis of water as evidence of all possible cases of water. The constitution of water, however exactly determined, is nevertheless as yet a purely empirical matter; so that we cannot be certain of the constancy of its composition. When the physical theory of matter has been carried a little farther than at present, and when we have a complete theory of the behavior of electrons and protons in their various possible configurations, we shall be able to see just what water is and why it must be composed of exactly a certain proportion of hydrogen and oxygen, and whether any other similar compounds are possible or not. The recent developments of physical theory seem to put such a complete theory at no great distance in the future. When we have such a rationally established chemical theory, we shall also be able to distinguish water from any other compound without dubiety, for we shall have a rational and exact (essential) definition of water, instead of a merely empirical and practical (nominal) one. It is no objection against the certainty of rational induction that chemical science has not yet been able to use it everywhere!

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[1] *Elementary Lessons in Logic*, XXVI, 223.

Jevons asks again, "Are we certain that the sun will rise again tomorrow morning as it has risen for many thousand years?"[1] We have seen already[2] that the rising of the sun is rationally established, with the exception that there are certain possible interfering causes which are as yet only empirically known. A collision with another body is a possible event, and the distribution of material bodies in our galaxy is not yet rationally predictable; when that distribution is rationally known, the possibility of a collision will either become an impossibility or else the collision will be predictable. Likewise the laws of the action of variable stars (of which our sun may be one) are as yet imperfectly known; when they are known as well as the laws of the motion of the planets are now known, the possibility that the sun might explode will also be either

[1] *Ibid.*, p. 224.

[2] Cf. chap. viii, § 3.

removed or made certain. The astronomical universe is at present a field of science many of whose laws are rationally established, while some are still empirical; the mixture of empirical elements with the rational laws renders some events, such as a collision with another star or an explosion of the sun, possible but uncertain. It should, however, be noted that neither of these events, were they to happen, would disturb our fabric of astronomic laws; either one would be quite in harmony with them. But that some such event should happen as the sudden stopping of the revolution of the earth—that is impossible; it contradicts rationally known laws; physicists have deduced that the force required would be impossibly large and that the stresses generated would be so great as to burst the earth and fling it apart in space. In so far as astronomical laws are rationally known, they are indubitable.

It has been said that science deals only with the *how* of things and not with the *what*.[1] Historically it is true that science began by describing those phenomena which are accessible to the senses; the first step in science is always the empirical generalization; rational induction does not come until later. By an appeal to the permanently valid work of Galileo and Kepler, it could be apparently proved that science deals only with the *how* and not the *what*. But Kepler did not stop with enunciating his planetary laws, he postulated agents to produce them—celestial spirits. Descartes likewise assumed his vortices in order to account for planetary motions. While these hypotheses were never verified, yet their enunciation shows that even at the beginning of physical science, the scientist was interested in the *what* as well as the *how*. The determination of the *what* has proved itself much more difficult than the *how;* the existence of such a scientific substance as the luminous ether is still a moot problem. Such a phenomenon as the long debate between the proponents of the one-fluid and the two-fluid theories of electricity should have shown philosophers that science *is* interested in the *what*, and the recent proof of the existence of the electron shows that it is possible to solve problems relating to the *what* as well as those concerning the *how*.

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[1] R. B. Perry, *Present Philosophical Tendencies*, p. 53.

Jevons' famous illustration of the ballot-box,[1] which has been used so many times subsequently to illustrate the fallible character of our knowledge of nature, is also based upon the supposition that empirical (i.e., enumerative) knowledge is the only form of induction. If the only way in which we can determine the comparative numbers of balls of each kind in a box is by successively drawing them, knowledge is limited to empirical enumeration; but a rational law spurns that method of proof: Jevons' illustration is misleading. The situation is in reality much more complicated than Jevons' ballot-box; instead of there being only one way of learning the nature of physical laws, there are many. A truer example would be to suppose a box with many kinds of balls, each kind of ball composed of a different metal, with a different specific weight, so that by weighing the box we can gain some idea of the proportion of balls of different metals; that we can introduce chemical reagents into the box, and, after withdrawing them, analyze them and determine the different substances in the box and their proportion; and that each kind of ball makes a different click when it hits the side of the box, so that by recording and analyzing the wave-lengths of sound obtained from rattling the box, we can tell the nature of its contents and the number of balls it contains—with this sort of a ballot-box, we could acquire a knowledge of the numbers and kinds of the balls in the box. By thus obtaining, in different ways, a verification of our hypothesis as to the contents of the box, we could be certain of its constitution, without ever drawing out any large proportion of the balls. A still closer analogy to the procedure of rational induction would be found if, from the appearance of the box, we could derive a hypothesis as to the purpose in the mind of its maker or designer (e.g., that it is a child's toy), and check our hypothesis by drawing balls from the box. Rational induction is different in its nature from empirical knowledge, and, unless inductive proof is different from that obtainable from Jevons' ballot-box, it is not rational.

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[1] *Principles of Science*, chap. vii, p. 150.

§ 2. It has also been claimed that in order to have certain knowledge of the contents of the ballot-box, its contents must remain unchanged. This requirement is an appeal to the uniformity of nature. It must be admitted that, in the present state of knowledge, this principle is as yet only an empirical generalization, and consequently does not admit of rational and certain proof. It is, however, one of the best established and most widely verified of empirical laws; its truth is presupposed in all natural science. The discussion of this principle will occupy us in a later chapter,[1] where we shall see what it means, to what degree it is capable of proof, and how such a proof could be made complete. The implication of the uniformity of nature in every physical law does render each law to some slight degree uncertain, as does the admixture of any other element which is only empirical; but that fact should not prejudice us against rational inductive proof in general—the fact that such proof cannot as yet be completed in the case of a fundamental metaphysical generalization does not make the method of rational inductive proof any less certain. This objection is not an attack upon the method of rational induction, but upon the results of its use.

All other concrete objections to the certainty of inductive processes are of one of the two types illustrated in the foregoing —either induction is conceived as proceeding solely by the establishment of empirical laws, or else some as yet merely empirical element (such as an unpredictable collision with a dark star, or variation from the uniformity of nature) interferes with our knowledge of the operation of rationally established laws. Jevons' objection to the certainty of the proposition that "gold is incapable of solution in nitric acid"[1] is due to the empirical nature of that law; the confusion of one substance with another is due to the uncertainty of chemical definitions of substances in the time of Jevons—Moseley's law has made possible an unambiguous identification of any chemical element. Euler's doubt that the stones of Magdeburg church would fall if unsupported[2] was likely based on the empirical character of the uniformity of nature; today no competent physicist would doubt such an assertion for any other reason. Lavoisier's assertion that all acids contain oxygen was a purely empirical generalization based on inadequate evidence. The failure of Lexell's comet to reappear, after having a period of five years predicted for it, turned out not to be a disproof, but a proof of Newtonian gravitation. These objections to the certainty of inductive knowledge misunderstand the logical nature of induction; they confuse the uncertainty of purely empirical proof with the certainty of rational inductive demonstration.

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[1] Cf. chap. xiii.

§ 3. An entirely different ground for the assertion that induction can merely give probability is found in the assumption that the only method of inductive proof is by the exclusion of rival hypotheses. Since we can never state exhaustively all possible alternatives, it is never possible to show that all possible alternative hypotheses are ruled out, so that certainty is impossible.[3] While science often does proceed by considering

[1] *Principles of Science*, chap. xi, p. 236.

[2] *Ibid.*, p. 238.

[3] Cf. F. C. S. Schiller, "Hypothesis," Vol. II, chap. xi, pp. 421 ff. of C. Singer, *Studies in the History and Method of Science.*

alternative hypotheses, and is often halted between two alternative hypotheses, which explain the known facts equally well, and while no crucial experiment is sometimes able for a long period to decide between the two, yet the conception of scientific induction as proceeding by a process of exclusion is a falsification of the facts.[1] Scientific proof is positive; the truth of a hypothesis is established by its verification, not merely by the disproof of other hypotheses. We do not find scientists following the method that this logical theory implies—racking their brains for all possible hypotheses, giving exhaustive lists of them, and disproving each one carefully! It is not necessary to exhaust reality before any hypothesis can be established.

§ 4. Still more trenchant is the objection that the mere fact that the phenomena fit in with the deductions from a hypothesis is no proof of the truth of a hypothesis.[2] Such a verification has been supposed to be committing the fallacy of affirming the consequent. We have previously seen that this fallacy is by no means committed when a large variety of deductions from a rational hypothesis are drawn and verified.[3] An examination of cases in which antecedents and consequents have been analyzed out, such as in chemical qualitative analysis, shows that a sufficiently wide deduction will enable us to be sure that one antecedent and one antecedent alone can account for the consequents that are found.[4] The meaning of the hypothetical proposition, that one consequent may be connected with a number of antecedents, is correct enough, but it is too great a simplification of the real situation; hence the method of verification of a rational hypothesis is not correctly represented by comparing it with the affirmation of the consequent in a hypothetical syllogism. Mill's discussion of this matter is a refutation of Whewell's assertion that the hypothesis of a luminiferous ether was verified, and in so far Mill was correct; indeed, that hypothesis is now successfully challenged. It was never widely enough verified; it explained the transmission of radiant vibrations and that alone. Whewell's conception of what is required to verify a hypothesis was defective. Not the mere correspondence of phenomena with deductions from a hypothesis, but the correspondence of an adequate number of phenomena with those deductions (adequacy to be determined in accordance with the standard of sufficiency that we have stated) is the criterion for the verification of a rational induction.

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[1] Cf. chap. vii, § 3.

[2] Cf. Mill, *Logic*, Bk. III, chap. xiv, sec. 6, for a good discussion.

[3] Cf. chap. vii, § 6.

[4] Chemical qualitative analysis and medical diagnosis are still empirical subjects; that is, the chemist has not yet discovered why some compounds should be gaseous and others insoluble, etc.; these disciplines are based upon the observation of mere uniformities, and we know only the two ends of the causal chain, not the intermediate links—that sodium chloride is a white crystalline powder, soluble in water—but not the connection between the antecedent, sodium chloride, and the consequents, white crystalline powder, soluble in water, ionizable, giving a yellow color to a flame, etc. Nevertheless, we can see that the

§ 5. Mill likewise objects that two conflicting hypotheses may equally well explain the known phenomena and that hence verification may be impossible. It is undoubtedly the case that we sometimes find two such hypotheses, as, for example, the one-fluid and the two-fluid theories of electricity. In such cases,

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presence of the antecedent results in the occurrence of a sufficiently varied set of consequents, so that the antecedent may be uniquely identified from the discovery of a sufficiently varied set of consequents; hence it can be proved that this antecedent, sodium chloride, and this *alone*, is present when the consequents are found. We are furthermore sure that the chemical characteristics of the elements are not the result of chance, but that a rational law, which will some day be discovered, is involved; while we do not yet know that rational law, we are sure that it exists; consequently, the as yet empirical character of such sciences as chemical analysis or medical diagnosis does not invalidate our proof. It is merely the result of the still unadvanced nature of science that the best available cases in which antecedents and consequents have been analyzed are as yet empirical.

science may be compelled to halt for a long time with neither verified, although adherents of both theories may each claim that their hypothesis is the true one. The reason that science is unable to decide between two such hypotheses is merely that a sufficiently large amount of evidence is not available; when enough facts become known, one or the other of the hypotheses is verified and the other disproved, as happened in the recent discovery and proof of the existence of the electron. Whewell's reply to Mill that as for all the known phenomena being accounted for equally well by two conflicting hypotheses, he knows "of no such case in the history of science where the phenomena are at all numerous and complicated,"[1] has been borne out by the development of science.

§ 6. It is also false that an exploded scientific hypothesis, if supplemented by a sufficiently large number of supplementary hypotheses, can be made to give a correct representation of the phenomena. The reason that this cannot happen, if a sufficiently wide range of deductions are made from the main and supplementary hypotheses, and if the known phenomena are numerous and complicated, is that a supplementary hypothesis, to clear itself of the suspicion of being purely *ad hoc*, must permit of further deductions in addition to the phenomena for whose explanation it is primarily invoked.[2] If a supplementary hypothesis does permit a wide range of such deductions (unless it is true), there are sure to be found some deductions that are contrary to experience.

It has been asserted that the Copernican astronomy depends solely upon the law of parsimony, and that the Ptolemaic system can never be disproved, for the reason that in calculating astronomical positions we use only the functions of sine and

[1] *Philosophy of Discovery*, p. 274; quoted by Mill, *Logic*, Bk. III, chap. xiv, sec. 6, p. 357.

[2] Cf. chap. vii, § 16.

cosine, so that everything that can be calculated by the use of those terms can be represented geometrically by means of epicycles.[1] Granting this statement as to the calculation of astronomical positions, does it follow that the Ptolemaic hypothesis is logically on a par with the Copernican (except for the superior simplicity of the Copernican view)? If we take the Ptolemaic hypothesis as anything more than a mere empirical statement of planetary positions, as a rational hypothesis, i.e., if we take it in any large sense, as, for example, meaning that the earth is the only center of motion, or that the motions of the heavenly bodies are uniform, or that all the planetary motions can be represented by eccentrics, deferents, or epicycles, superimposed upon circles, we find some contradiction between the theory and the facts. In the first sense, the moons of Jupiter and of the other planets contradict the view that the earth is the only center of motion. In the second sense, by accurate methods of measurement, we find such irregularities of motion (as, for example, in the moon) that it would be impossible to represent them by any combination of uniform motions. As for the third sense, such motions as the change in the latitudes of the planets occasioned Ptolemy great difficulty; he had to assume that the epicycles of the inferior planets pitched and rolled at the same time;[2] the precession of the equinoxes occasioned a similar difficulty—both motions are different in nature from a motion which can be represented by epicycles.

Hence it becomes impossible to take the Ptolemaic hypothesis in any other sense than as merely giving a statement of how the observed positions of the planets should be represented. As such, however, the Ptolemaic hypothesis becomes a purely empirical hypothesis, in that it permits nothing more than the deduction of positions of the planets similar to those observed. It gives no physical reason for the existence of deferents and epicycles, such as the presence of attracting masses of matter; the only reason for the existence and dimensions of these circles lies in the observed positions of the planets. It is accordingly unable to predict anything new; to account for the observed irregularities of the orbit of Uranus, new sets of epicycles would have had to be devised, and the planet Neptune would not have been discovered. It has furthermore all the marks of an *ad hoc* hypothesis: when a new irregularity is discovered, the theory must be changed, and an epicycle, larger or smaller, in accordance with the observed change in position, must be added for each irregularity. Logically considered, each new discovery must mean an overthrowal of the old theory and the formulation of an altered and logically distinct new one! Since every refinement in the accuracy of observations would thus have meant an overthrowal and change of the theory, the Ptolemaic theory would have been quickly seen to be merely an empirical formula and it would have lost the confidence of men, even if the Copernican hypothesis had never been suggested. In addition, there are several phenomena which would have remained quite unrelated, but which are taken up by the Newtonian formulation of the Copernican scheme, thus showing its power of superimposition—the tides, the precession of the equinoxes, nutation, the aberration of light (the latter perhaps the most convincing proof of the motion of the earth).

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[1] Columbia Associates in Philosophy, *An Introduction to Reflective Thinking*, p. 57.

[2] Cf. J. L. E. Dreyer, *History of the Planetary Systems*, p. 200.

In brief, if we take the Ptolemaic system as a rational hypothesis, it fails of verification; if we narrow it down until it fits the facts, it becomes an obviously *ad hoc* empirical generalization with no more wideness of application than the facts it was framed to explain. If the implications of any other exploded theory were likewise developed to a wide extent, they would also be found at variance with the facts, or the theory would be shown to be a mere empirical generalization which plainly shows its *ad hoc* character. By the application of the criterions of wide range of verification, non-verification of otiose elements, superimposition, and prediction, the false criterion of simplicity is rendered unnecessary.

§ 7. Venn has made the objection that, in the case of very many objects, especially the more complex ones, such as the waves of a stormy sea, two persons at different positions cannot see the same objects.[1] Without uniformity, how is verification possible? This objection shows that Venn has followed Hume in conceiving the inductive process as wholly analogous to the verification of a purely empirical hypothesis. If induction is solely the establishment of uniformities, and if uniformities are not observable, the situation is hopeless; but rational induction does not proceed by the establishment of uniformities; its proof is by showing that different events can be deduced from the same hypothesis. Under such circumstances, it is an advantage, not a disadvantage, that different observers see different phases (of the same thing), for thereby the basis of verification is made wider. The fact that I, standing by one side of a table, see it as a trapezoid, and you, in a different position, see it as an acute-angled parallelogram, does not prevent me from proving that its surface is a rectangle; it rather aids me, for I can show that both these figures can be deduced from a rectangle by the principles of projection. If a table-top always appeared as an acute-angled parallelogram from every angle of view, I could not establish any rational view as to its figure; it is precisely the diversity of the appearances obtained from the table that makes me sure that it is really rectangular; for they can all be deduced from the hypothesis that it is really rectangular, either by means of the geometrical principles of projection, or by remembrance of the appearances of an article that felt rectangular.

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[1] J. Venn, *The Principles of Empirical or Inductive Logic*, pp. 12 ff.

The rectangularity of such a table-top is due to far more than the mere dependence of mankind upon the sense of touch; that figure is the only hypothesis which explains *all* the facts, it is the only view that enables one to co-ordinate all the different visions of its figure with the impressions from the sense of touch. Touch is thus not more intrinsically reliable than sight; both are equally explicable by the hypothesis that the table is really rectangular. Uniformity of appearance is thus not necessary for the verification of a rational hypothesis; what is necessary is congruousness, i.e., the possibility of deducing different appearances from a single hypothesis.

§ 8. It has also been objected that "all laws and explanations are in a certain sense hypothetical, and apply exactly to nothing which we can know to exist. . . . . Physical astronomy is as hypothetical as Euclid's elements."[1] It is true that if we calculate the orbit of the moon on the assumption that only the attractions of the earth and moon are pertinent, we get an orbit which only roughly approximates the observed positions; in that sense physical astronomy is as hypothetical as a geometry which deals only with perfect circles and triangles. It is even true that, when the perturbing influence of the sun and other disturbing masses are calculated, the calculated orbit and the observed positions do not exactly match. The moon's motions have resisted the efforts of astronomers to a greater extent than the motions of any other heavenly body; nor has Einstein bettered the situation. With other planets, the Newtonian theory has been far more successful. But, granting all that, physical astronomy is not hypothetical, for when the effects of the influence of the sun upon the moon are added to those of the earth, the resulting orbit approaches more closely to the observed orbit than before, and the addition of the effect of each further perturbing influence makes the agreement of the calculated with the observed orbit closer. The process of approximation has not yet been completed for the moon as it has for some of the other planets. Nevertheless, astronomers are not describing the orbit of some hypothetical body other than the moon; they are endeavoring to account for the motions of the actual moon. In this case, nature has shown herself too complex; observations have been made so exact that scientists have not yet been everywhere successful in fitting a complex of hypotheses to the facts.

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[1] Jevons, *Principles of Science*, p. 458.

When Galileo enunciated his law of falling bodies, it was a merely ideal law; it is not true that all bodies descend with a uniform velocity, irrespective of their weight—feathers fall much more slowly than cannon-balls. But that does not mean that the law is merely hypothetical, in the sense that it is nowhere exactly obeyed—Galileo was merely neglecting one factor, the resistence of the air; when the air-pump was invented, it was found that feather and shot did fall with equal velocity in a vacuum. Galileo had no air-pump, but he saw that by neglecting a few cases of light objects, which the air might well affect appreciably, his law was obeyed. His law is therefore in no sense hypothetical; nature is usually more complicated than what can be expressed in a single law; the observed effects can often only be explained when more than one law is taken into consideration. The law is always obeyed, but its effects are masked by the influence of other factors.

Likewise, in enunciating his law of inertia, Galileo found that the more he eliminated the effect of friction, the farther a ball would roll; so he felt justified in asserting that if there were no friction the ball would proceed in a straight line indefinitely. Friction or the attraction of other masses can never be completely eliminated in any experiment made on this earth; but that does not mean that the law of inertia refers only to hypothetical nonexistent bodies—it refers to the actual bodies we deal with; the effect of inertia is masked, however, by the concurrent influence of other factors, such as friction, and so can never be exactly produced alone. Deduction is able to compose such different factors into a unitary effect; but that does not mean that the laws which are composed do not refer to any existent entities.

§ 9. It has also been objected that induction cannot give absolute accuracy, and that unless absolute accuracy is attainable, certainty is impossible.[1] The ratio between the diameter and the circumference of a circle can be determined to any required degree of accuracy, while it is by no means certain that the force of gravity does not vary with the 2.00016th power of the distance instead of with the square of the distance.

In the first place, it is to be noted, that in the case of any incommensurable, such as the ratio between the circumference and diameter of a circle, absolute accuracy can never be attained; consequently, by this argument, 3.14159 is not the true value of π, and we can never determine its true value! Certainly such a failure has not caused anyone to doubt the worth or truth of mathematics! Failure to attain to absolute accuracy does not invalidate the truth of any approximation that we have already obtained, especially when it is recognized to be only an approximation.

In the second place, we must recognize the difference between the nature of a mathematical statement, which is obtained by deduction, and a physical law, which is obtained by induction. It is a very fortunate circumstance that deduction can be carried out to as great a degree of accuracy as is required, so that deduction can always be more accurate than any verifiable consequence of an inductive hypothesis; for, by this means, accuracy of induction is made possible—if, in each step of deduction, there were always possible even a small error, these errors would mount up, so that long or complicated deductions would be of very doubtful value, and rational induction would be impossible. The absolute accuracy of deduction is thus one prerequisite for the certainty of rational induction.

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[1] Cf. Ward, *Naturalism and Agnosticism*, I, 139; C. J. Keyser, *Mathematical Philosophy*, pp. 361, 369.

It must be remembered that any result which is reached by deduction alone is merely the elaboration of what is contained in the premises; therefore it is merely hypothetical, and in no sense absolute; whereas an induced result is categorical and not hypothetical. The statement of the value of π ought to read: *if* there exists a perfect circle, and *if* it has a perfect diameter, and *if* the primitive propositions assumed for geometry (including metrical assumptions) are true, *then* the ratio between the circumference and diameter of a circle is 3.14159 (or whatever approximation is desired). Whereas the law of gravitation asserts categorically that the force between two bodies varies with the square of the distance (or approximately that amount); since that law has been verified by comparison with actual masses, this relationship is not contingent upon the further proof of the existence of masses, for they must exist before it can be truly asserted. Inductive laws are only hypothetical in the sense that any relation is hypothetical—the relation does not hold unless the terms are present; but a deductive or mathematical system is contingent in a far more profound sense—it depends upon the truth of the primitive propositions of the system; it stands or falls with them; and it has no veridical force of its own. It is merely the elaborated consequence of an originally unproved set of assumptions. The superior accuracy of a deductively obtained result is more than compensated for by its *wholly* contingent nature.

It is not necessary for a statement to be put into mathematical or numerical form in order to be accurate. As Huxley says,

> If I say that respiration is performed by the lungs; that digestion is effected in the stomach; that the eye is the organ of sight; that the jaws of a vertebrated animal never open sideways, but always up and down; while those of an annulose animal always open sideways and never up and down—I am enumerating propositions which are as exact as anything in Euclid.[1]

Compare that with the statement that the wave-length of the bright yellow lines of sodium are $.5896\mu$ and $.5890\mu$—a statement which may be in error a fraction of a $\mu\mu$; while the non-numerical statements we have quoted, just because they are not numerical, do not contain such a possibility of error! The real difference between the biological and physical sciences is not one of accuracy or of methodology, but that biology, since its subject matter is more complex than that of the physical sciences, is not so highly developed, and hence contains a larger proportion of merely empirical facts and a smaller proportion of rationally induced laws. That difference is not, however, essential, nor need it be permanent.

It is quite correct to say that empirical induction cannot give absolute accuracy. If the meaning of the term "induction" is confined to this method, the objection holds; the currency of the objection that induction cannot give accuracy is largely due to the fact that the natural sciences, in their present undeveloped condition, have been compelled to depend so much upon empirical induction. The progress of science means that rational induction will increasingly be used in place of empirical induction; the objection is invalid against a developed science. Nor is the use of mathematics necessary. We have long known that mathematical deduction is only one form of deduction; the logisticians have recently discovered other forms of deduction than the Aristotelian or the traditional mathematical logic; consequently, we are coming to see that we can use deduction in any field of thought, and thereby secure the accuracy and certainty of rational induction in any field of human interest.

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[1] T. H. Huxley, *Lay Sermons, Addresses, and Reviews*, p. 79.

The foregoing are the most important objections to the certainty of induction; they are all without force against rational induction, although they apply quite truly against empirical induction.

§ 10. It may be asked, How has it come about that natural science has acquiesced in the statement that science gives only probability? The answer is that it hasn't! Some scientists have been just as sure that their conclusions were certain as philosophers were that the conclusions in question were only probable. Often the scientist has merely assented to the philosopher's statement in order to escape metaphysical and logical discussion. The logician could always point to empirical laws in substantiation of his assertion that science does not provide certainty, and the scientist, because the distinction between empirical and rational laws had not been made, had no reply.

Science, however, has this characteristic—it does not need to wait for its hypotheses to be completely verified before it can use them. For its purpose, an incorrect hypothesis is almost as good as a correct hypothesis. The purpose of science is discovery; the scientist has found out that when he uses even an incorrect or untrue hypothesis to interpret his facts, he will usually be led to further facts, which may lead him to a better hypothesis. It was obvious that Faraday's tubes of force constituted a physically impossible hypothesis, yet they led to important discoveries. The same characteristic was shown by some of the other conceptions of that remarkably fertile genius. Impossible from the physical viewpoint when taken literally, yet they led him to highly important discoveries in electrical theory. He recognized their literal impossibility, yet used these analogies because of their fruitfulness. A false theory usually leads to the discovery of facts that overthrow it—the Ptolemaic theory served to sum up and interpret astronomical facts until sufficient facts were accumulated to overthrow it. Then the scientist does not need to deal with true and verified hypotheses only, much as he prefers them, but he can use any hypothesis, true or false, as long as it is fruitful. While his ultimate aim is the complete verification of his hypothesis, yet he realizes that such verified hypotheses, in the present state of science, are not many; so he is content to take a "working hypothesis" instead, as long as it will work. Hence it is quite correct to say that many of the hypotheses with which physics, astronomy, and other sciences deal, are only partly verified; in that sense they may be said to be merely probable ("possible" would be a better term); but it is by no means the case that all are only partly verified.

§ 11. Whether knowledge or certainty can be attained by the inductive method is partly a matter of the definition of knowledge. If we define the certain as that which cannot be doubted, it is, of course, not possible to attain certainty. It takes no great mental ability to doubt, and somebody will very likely always be found to doubt any statement, just as today some people doubt the rotundity of the earth. Even $2+2=4$ can be doubted. If by "absolute certainty" is meant indubitability, there is no certainty, not even that we have a correct definition of it! But if, casting aside as bootless such a definition, we distinguish knowledge or "absolute certainty" by the characteristic which some propositions have, that when they are thoroughly understood, together with their proximate and ultimate grounds (which includes the method of their proof), they compel the assent of every unprejudiced and experienced reasoner,[1] we have a nominal definition of knowledge which is useful and which represents the highest attainable degree of certainty.

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[1] Cf. chap. i, §§ 3, 5.

Deductive demonstration, whether by the Aristotelian syllogism or by mathematical deduction, cannot give certainty. For deduction can never demonstrate its ultimate major premises—it can merely assume their truth. Philosophy has demanded "absolute truth." Then what is meant by "absolute truth"? It cannot be literally indefinable, for we cannot think or speak about something of which we know nothing. Philosophers have usually meant by "absolute truth" demonstrative truth,[1] or valid deduction from true premises, i.e., a statement as true as $2+2=4$. We have, however, seen that deductive demonstration cannot give truth to any statement; it can merely communicate that degree of truth which is found in the premises. Shall we then demand that the premises of our deduction be "absolutely true"? In that case our definition of absolute truth becomes completely circular and valueless, for "absolute truth" becomes deduction from deductions which are deduced, and so on infinitely! Such an infinite regress makes "absolute truth" of this sort impossible. Yet this sort of truth or certainty is precisely what most logicians, ever since the time of Aristotle, have had in mind when they spoke of "absolute truth."

Deduction can at best only *communicate* that certainty which is inherent in its original premises. We have seen that the various methods which have been tried in order to secure certain premises for induction—coherence, a priori truths, necessary truths, authority, self-evidence, intuition, or mystical insight—are faulty, in that they need criticism.[2] Coherence becomes agreement with accepted beliefs; authority fails ultimately to sanction itself and needs to be founded on some other criterion; mystical insight is valid only for the person who has it, and fails to be uniform in all mystics; the a priori, the necessary truth, self-evidence, and other such criteria are misleading and unable to bear exact criticism. Jevons' statement that "certainty belongs only to the deductive process, and to the teachings of direct intuition"[1] represents a very short-sighted view of the problem of certainty; for deduction can *give* no certainty, but merely *communicate* it, and intuition is an *ignis fatuus*, whose alluringness lies in the remoteness of the ultimate intuited premises of deduction, and the failure of practical men to criticize their proof.

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[1] Cf. Ward, *Naturalism and Agnosticism*, I, 139: "We have . . . . not one *demonstrably* constant property of a physical description."

[2] Cf. chap. ii.

No one is compelled to accept the results of any method of reasoning. Certain sorts of argument—those used by "high pressure" salesmen—have an intrinsic psychological advantage because of their nature. Yet we learn as a result of experience with such arguments to discount them and even to be repelled by them. Carefully worked out logical reasoning does not have the intrinsic psychological persuasiveness of the methods of suggestion used by the salesman, yet we learn, as a result of experience, that we can submit ourselves with safety to the results of logical argument in a way that we cannot trust methods of suggestion. Then logical reasoning acquires power over us as a result of our experience with it—we submit ourselves to it if we are wise, and thereby it acquires, as the result of habit, a compelling power over us. The compulsive power of logic is the result of a (conscious or unconscious) value judgment based on experience.

Some people never fully surrender themselves to the power of logical argument, but cherish their own prejudices in spite of the strongest evidence and argument to the contrary. When a person carries to the extreme this procedure of refusing to submit to logical reasoning, we call him insane—the surest test of insanity is extreme unwillingness to submit to logic; so many people are, however, partly willing and partly unwilling to make this submission that it is difficult to draw an exact line between the wholly sane and the wholly insane.

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[1] *Principles of Science*, p. 267.

People also differ in the sorts of logical reasoning in which they place their trust. Some submit to pure deduction, taking mathematics as the ideal truth, not realizing its fatal weakness; others put their trust in purely empirical induction, and are convinced of the truth of astrology, palmistry, and other pseudosciences. We cannot trust such hasty judgments concerning the logical method in which we shall put our confidence; we can only admit that method which will draw the allegiance of those who have had the widest experience with logical methods, and which shows itself, as a result of experience with logical methods, to be beyond logical criticism—to be always reliable and never unreliable. Modern natural science constitutes exactly such experience with logical methods as is demanded by this criterion, and modern science has given its allegiance, often unconsciously, but nevertheless no less definitely, to the method of rational induction. The fact that this method has not been developed a priori, and has been accepted only as the result of trial and criticism, leads us to be all the more confident in it. The nominal definition of knowledge as that which brings about the consent of every unprejudiced and experienced reasoner leads to the essential definition of knowledge as that obtained by rational induction.

§ 12. Certainty cannot then be obtained by any of these popular logical methods except rational induction. They have each been rejected by some who have thoroughly understood their nature. Where, then, can certain knowledge be obtained? We are left with only one possibility—that of rational induction. Can rational induction give certainty? The best evidence that it can is the way in which views that were established by rational induction have swept over the world, and have gained credence everywhere, in spite of the severest opposition. Perhaps one of the most dramatic chapters in the history of thought is furnished by the victory of the Copernican world-view over the orthodox medieval view. The Copernican hypothesis was scorned by Protestant and Catholic theologians alike, criticized and ridiculed by the universities, and bitterly fought by the most powerful institution for the molding of life and thought that the world has ever known—the Roman Catholic church; the man who did the most to establish it on a solid foundation, Galileo, retracted and abjured it; and yet the Copernican hypothesis kept gaining adherents, until today there is not a single intelligent person who is unconvinced.

In the last generation this process has been repeated in the victory of the idea of biological evolution over men's minds, in spite of the bitterest opposition of the most influential part of the community—the pulpit and the great middle class. Even the power of the state has been invoked to stop its spread, but the very notoriety thereby given it causes it to be studied, hence to be better understood, and consequently to gain more adherents. It is exactly those that understand it best who are most willing to confess its limitations, and who are also most certain of its truth. The victory of the Copernican world-view, which began when Galileo showed that it was able to explain such widely different facts as the moons of Jupiter and the phases of Venus, was not merely the victory of a new world-conception—it was the victory of a method of demonstration, so ancient that it had become almost instinctive, but newly applied to philosophic and scientific matters in place of the traditional Aristotelian method of deduction. No wonder that it inaugurated a new era in science!

In these and many other instances, the method of rational induction has shown its power to compel all open-minded men, without exception, to accept its results in proportion as the grounds of those results have been thoroughly understood. If this is not certainty, what is? Can we ask for any better proof of the certainty of any logical method? Furthermore, the certainty attained in rational induction comes not by a non-logical process, such as intuition or mystical insight, but by a process that is thoroughly logical, criticizable at every step. Rational induction has validated its claim to be a sure and certain method of attaining truth or knowledge.

It is, of course, true that some scientific laws are only probable. Our contention is not that every candidate for rational verification has established its case; there are today a great many scientific "laws" which as yet lack complete confirmation. In many cases it is impossible to decide between alternative hypotheses; and in all cases our truths are only limited universals, whose limits may be inadequately perceived. We must remember that even mathematical truths, such as $2+2=4$, are not universally true. These truths are true *only where they apply.* So with all the other results of rational induction—they are true only within limits. It is likewise possible that the best established of our present rationally verified truths may have later to be slightly modified—we have already seen that any verified proposition may be found to be only a limited universal; but we have also seen that no verified law is ever abrogated; it may be found to be only an approximation, but not to be false. The determinations of the specific weights of such chemical elements as possess isotropes are as much used today as they ever were, although today we see that these determinations were merely empirical generalizations, averages of the weights of different isotropes; the reason for the existence of these isotropes will doubtless soon be established as a deduction from some rational law regarding the composition of the elements. Newtonian physics is today asserted by some to be a special case of Einsteinian physics; but Newtonian physics is not abrogated; it is still as true as ever, and used as much as ever; its application is merely limited to cases of low velocities. The fact that a proposition is a limited universal does not rob it of its certainty; the limited universal does not cease to be certain because of its limitation, it merely ceases to apply as universally as was supposed.

§ 13. It is, of course, true that some people have doubted even such well-established truths as the approximate rotundity of the earth. Such people are either the ignorant, who do not understand the grounds upon which such a truth is established, or the prejudiced, who are unwilling to accept any conclusions at variance with their own beliefs, or the oversophisticated, especially the logicians, who do not understand the method of demonstration used and so conclude that even such a hypothesis as the rotundity of the earth is merely probable. The rotundity of the earth is not evident to the senses; it is not a normal mystical intuition, nor can it be deduced from such an intuition, hence it is thought to be merely probable. Those who take this view are especially the followers of the Humean conception that induction is merely the establishment of empirical laws. These logicians must remember that no other method of attaining truth has been found to give ultimate certainty; unless certainty is somewhere attainable, all probabilities reduce to probabilities of probabilities and everything becomes equally improbable.[1] If we deny the certainty of induction, it becomes *equally* improbable that the earth is flat and that it is round—no difference can be made between the probabilities of the most absurd and the most reasonable statements. Square circles and round triangles become equally probable with the best demonstrated of mathematical laws. If certain induction is impossible, the primitive premises of mathematics become merely probable, and the whole of mathematics becomes also merely probable. Unless certainty is attainable by induction, it cannot be attained by any logical process, and all probabilities become improbabilities. Such are the consequences into which those attacking the possibility of certainty are led.

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[1] Cf. chap. iii, § 28.

If certainty is unattainable by rational induction, then we must say that even the statement that "my table is rectangular" is not certain, for that assertion can only be established by induction. We must likewise say that such a statement as "This is a house" is not certain, for such perceptual judgments are also validated only by an inductive process.[1] Furthermore, we must say that the statement "Your view is uncertain" is also uncertain! Contradictories become equally probable, and not even "right opinion" is possible, for it is equally improbable with "wrong opinion"! This is the logical situation into which the opponent of the view that truth can be attained by induction is thrust, when he carries out the implications of his position; for we have found that no other logical method can give certainty, unless induction can do so. A fearless logician will not be deterred by such an awful alternative from accepting the consequences of his skepticism; but it should at least give pause to the critic of induction to find that, as a result of his view, even his own denial of the worth of induction cannot be certain!

Rational induction may thus be considered to have made good its claim to be the method which attains to certainty, knowledge, and truth, since it has shown that it satisfies the most rigorous of possible definitions of knowledge by its compelling power over unprejudiced and experienced reasoners, and that it is able to silence its critics by pointing out the implications of their own position. We shall accordingly proceed to consider the still deeper question, whether rational induction can establish its own truth.

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[1] Cf. chap. vii, § 4; chap. xi, § 2.

CHAPTER X

THE DEMONSTRATION OF THE RATIONAL INDUCTIVE METHOD

§ 1. We must next proceed to the task of justifying our account of rational induction as the method of attaining truth or knowledge, which we had to interrupt at the beginning of this part of the book, for the purpose of giving a description of that method and a reply to some objections made against it. We must now resume our critical attitude toward the method of rational induction, and ask what proofs of its truth and correctness it can bring forward.

It is important that a logical method should justify itself as correct. Otherwise, since logical method is the foundation of all proof, philosophy would begin by making a fundamental assumption, which might vitiate its whole procedure. Indeed, it is a notorious fact that the idealistic philosophy is founded on a particular logical method, which is assumed and not proved, and that, when the logical method is given, the idealistic metaphysic is predetermined.[1] Consequently it is necessary that every ultimate logical method should demonstrate[2] itself.

What method can we use to justify the method of rational induction? Our thesis is that it is the ultimate method; the use of any other method than it to establish itself would imply that this other method is more ultimate than it. Consequently, we can use only the inductive method to justify itself. That means that we must take the inductive method as a hypothesis and then use that method itself as the means of testing its own truth. Yet if we use the inductive method to demonstrate itself, are we not then prejudicing the issue, and are we not committing the fallacy of *petitio principii?* This point must be examined, for it will not do to make such a mistake at the outset of our demonstration. We shall consider the second objection first.

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[1] Cf. the remarkable confession implied in the following statement, which is found at the beginning of a widely used text on metaphysics, "The very nature of the metaphysical problem predetermines the general character of the answer we are to give to it. What our intellect can accept as finally real, we saw, must be indissolubly one with actual experience, and it must be an internally coherent system" (A. E. Taylor, *Elements of Metaphysics*, p. 50).

[2] Using the word, demonstration, in the sense of a complete proof, whether by induction or deduction. Cf. chap. i, § 4.

§ 2. We have seen, in discussing deduction,[1] that there are unobjectionable and objectionable *petitios*. It is our contention that the use of the inductive method to justify itself is an unobjectionable *petitio*. The reason for this assertion is that, in assuming the inductive method in order to prove itself, we are not assuming it as true, but only as a hypothesis. If the inductive method succeeds in proving its correctness, then we shall accept it; but if it does not verify itself, we shall reject it. It is a characteristic of hypothetical induction that it constantly makes assumptions for the purpose of testing them. The hypothesis must be given *before* it can be established. But the hypothesis is not given as true and established; it is *considered* as a candidate for truth, which is encouraged to put forth all its powers, but not accepted until it has been tested and found worthy. There are more than two truth-qualities possible—a proposition may be taken as true, or as false, or it may be merely considered, without asserting it to be either true or false. It is with this third truth-quality that a hypothesis is put forward. Consequently, we are not committing any fallacious *petitio* in using the method of rational induction both as a hypothesis and as a method of testing its own correctness.

A further characteristic of hypothetical induction is that a method of deduction is assumed along with the hypothesis, whereby the hypothesis is to be developed. It is consequently not a departure from the usual procedure of induction that we assume a method of testing the validity of the hypothetical method as part of the hypothesis itself. Whenever we choose any method, inductive or deductive, to test the truth of a proposition, we are really doing no more than we are doing here. Hence we are not committing a fallacy in using the inductive method to prove its own truth.

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[1] Cf. chap. vi, § 28.

It is furthermore to be noted that a circularity of this sort is inevitable in any attempt to establish a logical method which shall be ultimate. Either we must take our ultimate method without testing it, or we can test it only by itself. Hence this particular situation is unique, in that circularity is inevitable, whereas, in any other situation, it may be avoided.

§ 3. A consideration of the consequences, for other theories of knowledge, of the situation which thus arises at the logical beginning of demonstration, is not amiss at this point. If we must begin by using a method to substantiate itself, it is easily seen that the deductive method cannot deduce its own truth; for deductive demonstration can only be attained by finding a set of true premises from which a proposition can be demonstrated; but if the deductive method is the ultimate method of attaining truth, no knowledge can exist before the deductive method has demonstrated itself to be true, and hence there can be no true premises upon which the deductive method can base its demonstration of itself! It is not surprising, therefore, that J. S. Mill should have asserted, "Questions of ultimate ends do not admit of proof, in the ordinary acceptation of the term."[1] He was conceiving of ultimate proof on the analogy of deductive demonstration alone. The deductive method is derivative and not ultimate, for it cannot demonstrate itself.

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[1] *Utilitarianism*, chap. iv.

With the intuitive method, the situation is otherwise. It is quite possible that the intuitionalist should have an intuition that the intuitive method is the correct method. While intuitions of this sort have not commonly been recorded, yet that sort of intuition is implied in the confidence which the mystic has in the veridical character of his mystical experience. Thus the intuitive method is able to substantiate itself. Nevertheless, such intuitive verification of itself is of the same character as other intuitions—it has no logical value for anyone except for the person who himself has that intuition. For anyone else, the acceptance of an intuition that he has not himself had, becomes an act of acceptance of authority; and authority is never logical because it is never ultimate; it depends on some other evidence.[1] Intuition cannot subject itself to logical scrutiny without admitting that some other method is more ultimate than itself, for criticism of intuitions by intuition is impossible. Intuition can only be scrutinized by reason.

Thus, of the possible logical methods, only induction can logically substantiate itself. Deduction is unable to deduce itself; intuition may intuit itself but cannot criticize itself; only induction can induce itself and at the same time criticize the process whereby that induction is accomplished.

§ 4. We must furthermore inquire whether the use of induction to prove itself does or does not prejudice the issue. One of the advantages of the inductive method is that it does not of itself lead to any metaphysical conclusions. Monism or pluralism, idealism or realism, could be equally well established by the inductive method, if the requisite facts were produced.[2] The issue would only be prejudiced if the case were stated in such a way that only one conclusion could be drawn. A careful analysis of our argument as outlined here will show that it is quite conceivable that the inductive method should give a negative verdict upon itself, since the appeal is not to the logic of induction but to its *results*. Were the results other than they are, an entirely different valuation of induction would be inevitable. Indeed, from the nature of the argument whereby the reliability of the inductive method is established, it can be seen that the ancients were precluded from demonstrating its reliability, because they did not have the facts which we have now, and that, while modern philosophers and logicians could have perceived the nature and proof of rational induction many decades ago, yet it is not surprising, in view of the comparatively recent development, rapid advance, and wide interest in natural science, that the recognition and demonstration of the nature of inductive method should have had to wait until our century. The foregoing considerations indicate that the inductive method is free from obvious logical objections, and that it is the only logical means whereby any method could establish itself by a critical procedure as the ultimate way to knowledge.

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[1] Cf. chap. ii, § 8.

[2] Cf. chap. vi, § 30.

§ 5. We shall then take as a hypothesis the method of rational induction, as we have stated it in the previous chapters, and ask ourselves what deductions we can draw from the supposition that this is a correct method of attaining truth or knowledge. Our deductions are three: first, the use of this method ought to account, in a harmonious fashion, for the complexities and paradoxes of scientific methods; second, its use ought to have resulted in the attainment of what is evidently knowledge; and, third, this method ought to account for the various misconceptions of the method of knowledge which have been popular in philosophy. Only a correct method of knowledge can do all of these things. The basis on which we make these deductions is that the use of a correct method ought to produce knowledge, and that the discovery of *the* correct method ought to prove to be that for which, in the past, investigators into the nature of philosophic method have been really seeking, but which they, for one reason or another, have misconceived. If it is asked how we know that a correct method ought to produce knowledge, we reply that what is meant by the term "correct method" is that it is one which produces knowledge, and that logicians and philosophers have stated that they were seeking knowledge and the method whereby to attain to it. Thus our deduction is based on a definition, and on the statements of philosophers about themselves; it could be put into strict syllogistic form, if necessary.

§ 6. The proof of the first deduction, that the correct method of knowledge ought to account for the difficulties and paradoxes of scientific method, has already been given in this part of the book. Our account of the method of rational induction has had two foundations: first, the logical one that where the consequents of any set of antecedents are complex and varied, it is possible to pick out an antecedent unambiguously by a consideration of its consequents;[1] and second, the actual phenomena of the concrete use of scientific method. The foregoing chapters have shown how well these two sources of our account fit together; we have seen that an account of induction which requires for certainty primarily nothing more than a wide range of verified deductions from a hypothesis is able to unify the various features of scientific method into a harmonious account, and to evaluate those features in terms of that single requirement. Inasmuch as we have drawn our illustrations of the use of rational induction from a very varied list of sciences, we have thus already achieved a wide range of verified deductions from our hypothesis. In so far the whole second part of this book is a proof that we are right in asserting as correct the method of rational induction herein outlined.

The substantiation of the second deduction, that the use of this method ought to have resulted in the attainment of what is patently recognized as knowledge, has also been already given. The method we have been discussing is not one that is new and untried—a Baconian new method— but the one that has been intuitively used by modern science in substantiating its discoveries. The many illustrations we have given of the use of this method show that we have not only been drawing out the consequences of the assumption that in a complex world it will be possible to identify an antecedent by its consequents, but that at the same time we have been analyzing the method which has really been used by science in substantiating its discoveries. It is impossible to escape confidence in a method which has brought about such a revolution in human life as natural science has done. If the pursuit of science had merely resulted in a set of abstract laws for the contemplation of the scientist, we might not be so confident in the truth of its results; but inductive science has been turned to practical use, and the inventions which have resulted—the electric light, the radio, the aeroplane, and a multitude of others—are an eloquent reminder that science must have attained to truth, for only truth could have enabled the production of so many wonders. Science is not mere contemplation of reality or statement of observed uniformities of experience; science is interaction with nature, in which questions are put to nature and replies are secured from nature until she is unambiguously understood. Science has used experiment; but patient observation can be equally well turned into careful interrogation of nature, and the results attained can often be used for that manipulation of nature whereby our marvelous modern civilization has been attained. The success science has achieved by the use of the inductive method is the proof of its correctness.

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[1] Cf. chap. vii, § 6.

If, on logical grounds, the Humean positivistic objection is made that natural science has only attained probability and not certainty, the reply is patent: Can probability bring about skyscrapers, submarines, the trans-Atlantic telephone, the extermination of yellow fever, and other such results? Can probability compel men to believe in a theory such as that of Copernicus or of biological evolution, against their will and against the pressure of the most powerful institutions of society? If probability can do that, then philosophers had better cultivate probability in order to end the disagreements which Kant recognized were disfiguring philosophy and preventing her from having attained to the "secure method of a science." Our contention is not that every deliverance of science is knowledge, but that the substantiated and well-tested deliverances of science are certain. Even the most rabid relativist has not doubted that the earth revolves around the sun. As soon as men began to know anything about astronomy, they began to suspect that the earth is not flat and even that it does not stand still; but these were only undemonstrated opinions, and so had no compulsive power; the view that the sun is the center of the solar system had to push its way into the minds of men in spite of Scripture, in spite of its supposed materialistic and antireligious implications, until today it is universally accepted among intelligent men. Only truth can do that.

§ 7. The foregoing considerations alone are sufficient to establish the correctness of the method of rational induction; that method which can be shown to have been used to substantiate completely only one piece of undoubted knowledge is thereby shown to be *a* correct method; however, in order to widen the range of verification still more, and to show that rational induction is *the* method of knowledge, we shall undertake to show that the characteristics of this method account for the various logical theories which have dominated human thought, and that each of them is just one aspect of the one complete method—rational induction. The argument, which our criticism of other logical methods offers, will then be complete, that this is the *only* ultimate method of substantiating the truth. We shall undertake this proof only for the general methods whereby men have supposed that they attained truth, and leave it to the reader to supply the detailed application to various historical theories.

The rationalist's deduction is now recognized to be one of the most important features in the method of rational induction. The function of deduction is the elaboration of a hypothesis to the point where its consequences can be compared with the facts of experience. This function of deduction accounts for precisely those characteristics which are most perplexing when it is taken as the sole method of proof—of which the most striking is its inability to provide its own premises. These premises must be provided by the hypothesis. If the deductive method provided its own ultimate premises, it could not be used impartially for the elaboration of both true and false hypotheses; but, since no ultimate premises are deductively substantiated, the deductive method can be applied to any hypothesis. Likewise, the inability of the deductive method to justify its own procedure is a consequence of its being an integral part of the inductive method—that justification is given by induction. Deduction is not an independent method, but only one aspect of a larger whole.

The inability of deduction to justify its canons is also due to its dependence upon induction; for these canons are ultimately derived from the sequences of experience and the conventions of language, and are only justified by induction. The inability of deduction by itself to produce truth is also accounted for—deduction gives only *valid* results, in order that a false hypothesis can be elaborated as correctly as a true one, and that the dubiety of the false may be evidenced as well as the truth of the true. If deduction by itself gave truth, there would be no rational way in which to elaborate a false hypothesis and thereby to disprove it! The accuracy of deduction is also explained—if each step of deduction were not absolutely accurate but were inaccurate even to a slight degree, those inaccuracies would accumulate, until, for long chains of deduction, the probable error of the deduction would be so great that the correspondence of its results with observed facts would have no evidential value. Thus it would be impossible to establish those larger generalizations, such as Newton's laws, which demand long deductions. Even where absolute numerical accuracy is impossible, such as in the calculation of incommensurables, deduction is able to develop its results beyond any given degree of accuracy, and so deduction can always be *more* accurate than observation, no matter how accurate the instruments of precision are made—thus providing a means of carefully checking any hypothesis. Deduction is therefore perfectly suited to the development of hypothesis and to its place in the inductive method; the very anomalies that militate against its use alone make it exactly suitable for use in induction. Deduction is one of the most important features of the inductive method, and consequently it is not surprising that some philosophers, in an incomplete analysis of scientific method, should have selected deduction as the fundamental method of gaining knowledge.

§ 8. What then is intuition? Intuition is the method by which all great discoveries have been made, though none of them have been established as true by this means. It is a psychological phenomenon that all great discoveries, or almost all of them, have come to the discoverer in a flash of insight after a long period of brooding over the matter. Poincaré has given a good illustration and psychological description of the intuitive operations of the mind.[1] As he shows, all the valuable mathematical results which he discovered came to him in flashes of insight. Yet he also had insights that revealed untrue propositions.

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[1] *Science and Method*, Bk. I, chap. iii, "Mathematical Creation."

All great discoveries have probably come in this way. There are, of course, individual differences; for some people, conscious thinking plays a larger part than for others; but in all cases the crucial insight comes in a moment. Some put more emphasis upon the sudden insight, and others upon the conscious process of thinking, which invariably precedes it; so some stress intuition more than others and allow themselves to be guided by it to a greater extent. Not only in science and philosophy, but in practical life, the man who has insight or good judgment reaches decisions which are correct, though sometimes he is unable to give reasons for his decision.

Intuition is thus a pervasive phenomenon; it is the means whereby discoveries have actually been made; consequently, it is not surprising that some philosophers should have relied upon intuition for truth. It does sometimes give truth; but even geniuses have made mistakes and have wasted valuable time in bootless endeavors. Intuition is a most valuable possession for the man who has it; but its results always need to be criticized by logical processes, or else it will lead to error as well as to truth.

§ 9. Authority is another criterion of truth which has resulted from a misconception of the inductive method. Reliance upon authority is the attitude that most people must take towards most pieces of knowledge. The process of rational verification is so complex and takes so much time that only a very small proportion of the population can ever take the time to examine and verify for themselves any large number of the sure results of science. Consequently, the only thing that most people can do is to accept the authority of someone who knows better than they do. Even the revolution of the earth is probably accepted by most people in this fashion, for they cannot stop to make the accurate observations, learn the mathematics, and perform the calculations necessary to verify the Copernican scheme. The mathematical difficulties in the way of verifying such phenomena as the precession of the equinoxes, nutation, the aberration of light, etc., are sufficient to exclude all but a very few from ever attempting to understand fully many of the details in astronomical verifications. All that most of us can do is to understand roughly the general features of such a proof and trust the astronomer for the rest. Reliance upon authority is a very real and important phenomenon of science today.

Yet the authority of science is neither institutional nor esoteric. The only prerequisite of an independent investigation for the purpose of testing any scientific knowledge is the necessary time and equipment, together with a full understanding of the method and subject matter. Science invites all who can understand the subject to investigate for themselves, and relies only upon the persuasive power of the subject matter for unanimity. But those who cannot take the time or who cannot understand the subject matter can only take the authority of others who do. When we remember that the inductive method has been used ever since men began to think and reason, though often without an understanding of its nature, it is not surprising that authority should have been taken by some as the method of knowledge.

There are two other reasons which aided the method of authority. When reliance is placed upon intuition, the acceptance of the results of intuition is only intuitional for the person who himself has the intuition; for others, the reliance upon his intuitions becomes a reliance upon his authority. In this way the reverence of the multitude for the insights of the genius becomes a reliance upon the authority of the genius. This attitude was often crystallized into a doctrine of revelation; for the insight of intuition does often seem to come from an outside source.

On the other hand, the influence of authority has sometimes been accentuated by a misunderstanding of the inductive method. The inductive method consists in boldly putting forward a conclusion, and then substantiating it by deducing and checking its consequences. To one who does not understand the nature of inductive method, and knows only of deductive reasoning, the inductive process naturally appears to consist in an authoritative enunciation of a fundamental premise, together with the development of a philosophical system from it. Exactly that sort of misunderstanding ensued when Kant used the inductive method to substantiate his doctrine of the categories, and called it a metaphysical and transcendental *deduction*. His critics called him dogmatic; provided we understand his argument as merely deductive, that criticism is quite just, and it is not surprising that his successors should have been similarly dogmatic and authoritarian—they were merely imitating Kant's method. Such misconceptions of the inductive method are quite natural; authority has a necessary and useful function to play in the practical use of induction; but it has its limits—rational induction does not justify *mere* authority, but authority supported by reason, together with freedom of investigation and belief.

§ 10. Faith is another method of knowledge that has been much emphasized by some, especially by religious leaders. To some, reliance upon faith has meant what we have meant by intuition—direct insight into truth. To others, faith has meant acceptance of authority. Yet faith is more than that: it is the determined clinging to a position which is not established by any logical procedure, sometimes in the face of great difficulties and virulent attacks—a thoroughly non-logical procedure. It has been decried as illogical and superstitious, or praised as the fundamental requirement of religion, and some pragmatists have made it (in the form of the will to believe) the way to such knowledge (better called right opinion) as can be had.

Faith is a moral quality of the highest importance to the inductive method. Inductive verification is often a very arduous procedure, a road that the faint-hearted can rarely pursue to the end; faith is the psychological attitude towards a hypothesis that must be taken by an investigator before he can hope to go through the arduous task of verifying it. He may be wrong; if so, he may waste years or even a lifetime; yet such risks must be taken if knowledge is to progress, for, without taking them, nothing will be achieved.

Faith in one's hypothesis is thus of the utmost importance, and must be cultivated by the scientist as well as by the religious man. Nevertheless, it should be a canny faith and not a blind faith, a faith that is guided by reason and good judgment, as far as possible, if it is to attain its goal. Probably the outstanding example of such faith is Kepler's long and arduous search for his mathematical laws of planetary motion. His faith was grounded on an erroneous foundation—the simplicity of nature; if it had been weaker, or backed by a less adequate mathematical equipment, it would have failed; it was the very persistence of Kepler in spite of the previous failure of the *Mysterium Cosmographicum*, his dogged faith in his thesis that nature is statable in mathematical terms, that led to his brilliant success. The importance of faith for science can hardly be overestimated.

Faith is even more important for practical life. Science can wait to achieve knowledge; practical life demands action; inaction is equivalent, in practical terms, to denial. Science is very slow; matters of the most importance for practical life, such as the existence of God, cannot be dealt with at the beginning of science. Life demands action—there is, under such circumstances, no alternative between belief and disbelief; agnosticism is equivalent, for most practical purposes, to disbelief. Then, whether we act as if God exists or as if he does not exist, we are equally basing our action upon some sort of faith. Faith thus becomes the attitude to hypotheses that, in the present state of knowledge, are unverifiable, an attitude which the practical exigencies of life force upon every man. Life compels us to choose, whether we wish to do so or not. If we do not have faith in the existence of God, then we have faith in the practical nonexistence of God. Under these circumstances, the appeal to the will to believe is thoroughly justified. Risks must be taken, and he who chooses wrong may have to suffer bitterly for his mistake.

This faith is not, however, a faith which goes contrary to reason, so that a thing may be true for faith but false for reason (i.e., for inductive verification). Where knowledge is had, faith has no place. Instead, faith supplements knowledge in matters upon which inductive reasoning has not been able to render a decision. Faith may refuse to accept an argument that is not conclusive; it may go contrary to popular opinion or even to the "weight of scientific evidence," however that ambiguous phrase is interpreted; since evidence, even if it is called "scientific," has no compulsory power until it is conclusive. Faith may rightly refuse to accept such unscientific speculations as "scientific naturalism" or materialism; but faith dare not oppose knowledge. Those verifiable deductions that can be made from faith's hypothesis should correspond to experience, not contradict it. Logically considered, faith is justified because it rests upon the belief that its postulates will or may sometime become part of knowledge; in view of the urgency of life, faith makes its decision in advance of knowledge. Faith is the means whereby men supplement the deficiencies of knowledge in matters of practical urgency. It is also a psychological prerequisite for the arduous task of inductive verification. It is, consequently, an important part of the inductive method.

§ 11. The coherence theory, which depends upon the system-making power of knowledge for the criterion of genuineness, is also due to the mistaking of one phase of the method of rational induction for the whole. Since a hypothesis, to be verified, must provide a wide range of verified deductions, carrying its deductions into more than one field, a true hypothesis inevitably generates a harmonious system through the power of deduction. Hence it is correct to say that every truth creates a system; but it is not correct to say that every system implies a truth, for falsehoods create systems by deduction as well as truths. The difference is that the system created by a falsehood fails to verify itself when confronted with the facts of experience, whereas that created by a truth does verify itself. One important difference between the rational law and the empirical law is that the former is able to form a system by deduction, whereas the latter is not. The coherence theory has thus seized upon a vital element in rational induction, though not the whole. Coherence is a characteristic of truth, but not a valid criterion of the truth of any proposition.

§ 12. Speculation has also been taken as the way to knowledge, though philosophers have rarely depended upon speculation alone. Speculation, too, has its place in scientific induction. Speculation is the suggestion of possible hypotheses. As such, it has an important function in the inductive method, for we have seen[1] that the most difficult of all the steps in induction is often the statement of a hypothesis. This is the step that oftenest waits for a genius. Hence the enunciation of hypotheses that seem to fit the case is sometimes a very valuable piece of work. It is, of course, true that "mere speculation," the constructing of air-castles which fit nothing, is useless. The speculator has the duty of at least attempting the verification of his hypothesis; if he cannot deduce anything from his hypothesis which corresponds to any of the facts of experience, he had better keep still. It is only possible speculations that are worth while—the suggestion of hypotheses that seem at least to fit a large number of facts. Even if such a hypothesis is not true, the disproof of it may lead to advances in knowledge. The nebular hypothesis was worth while, not because it was a vast speculation, but because it did seem to fit the facts of cosmogony. If today it is shown that the deductions made from the hypothesis are unjustified, at least we shall have learned much of positive worth in disproving it. Speculation, when it is accompanied by at least partial verification, is extremely valuable in the progress of science. It is a preparatory step for the use of rational induction.

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[1] Cf. chap. v, § 4.

Hence it should not be supposed that the demand for the use of rigorous inductive reasoning will banish speculation from philosophy and restrict science to "facts" and "facts" alone. Such has been far from the case when the inductive method has been used. The most active speculators today are the inductive scientists. It is often true that no sooner is a new phenomenon known than half a dozen theories are proposed to fit it! When a reputable twentieth-century astronomer can speculate upon the effect of the moon on the growth of vegetables and lunacy,[1] surely no philosophical or religious speculations can be interdict! Speculation is not only an innate tendency, an effort to satisfy the instinct of curiosity, but also, when carefully used, a valuable aid to the growth of knowledge, because knowledge only comes by the verification of speculations. Induction does not stop the use of speculation; it puts speculation into its proper place, as the suggestion of possible hypotheses.

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[1] Th. Moreux, *Astronomy Today*, pp. 98 f.

§ 13. The pragmatic criterion of truth, that a proposition is true because it "works," is another misconception of the method of rational induction. The great influence of pragmatism has been due to its seeming adherence to the method which has been used with such success by natural science. The failure of pragmatism has been that it failed to understand correctly the scientific method of rational induction, and so summed up the criterion for the verification of a hypothesis in the highly ambiguous phrase, "It works." That phrase may mean anything or everything; a dream "works" just as much as a real experience; an untrue view of a situation, which satisfies my vanity, "works" just as truly as a correct view. If pragmatism had endeavored to define carefully what is meant by "working," it would have been able to meet its critics successfully; but it failed to state positively what constitutes "working," and therefore justly laid itself open to attack. Pragmatism is a vague attempt to reach after the method of rational induction.

§ 14. All the various major methods of reaching knowledge are, in fact, misconceptions of the method of rational induction. This method puts each one in its place and remedies the defects of each by supplementing it. Rationalistic deduction, intuition, authority, faith, coherence, speculation, and pragmatism, all find their mutual supplementation and their proper place in the method of rational induction. In this fashion, the method of rational induction is the true synthesis of all other methods. Our proof of the correctness of the rational inductive method therefore meets the test of superimposition. This synthesis is all the more remarkable as an evidence of the truth of our theory in that we did not set out purposely to synthesize all existing methods, but to seek the truth by a destructive criticism of existing methods; when we found the truth, we also found that it synthesized the very methods we had criticized and rejected. Truth alone can thus synthesize the various attempts men have made to reach it. This synthesizing power of rational induction is not only a proof that induction is a correct method of gaining knowledge, and that the method, as we have stated it, is correct, but also a proof that it is the only method of reaching ultimate knowledge. We have considered all the other alternative hypotheses which might present a possible method of knowledge; they have all been shown to be unable to reach it; this method not only does reach knowledge but it also utilizes the worth-while features of other methods, and forms them into a new whole.

Rational induction thus proves itself to be the correct method of knowledge by a test that is thoroughly critical—something that no other method can do. If we should be asked to meet the further criterion of prediction, it would seem that if we have attained the right method of knowledge, the result should be a revival and progress in philosophy. Whether this will be the case or not we cannot say, since the field of philosophy is peculiar, in that its material consists of conceptual constructions rather than the observed phenomena of nature; the field of conceptual construction has been so thoroughly worked over in the last two milleniums and a half of philosophical investigation that new discoveries are hardly to be expected; and, if by progress in philosophy is meant new conceptual constructions, such are not to be expected. Nevertheless, philosophy, by the application of this method, ought to be able to determine what is certain and what is doubtful in philosophical theories, and to begin the task of filling in the weak places in proofs of philosophical hypotheses. In this manner we may expect some results to be definitely established as proved, and at least a gradual, though slow, progress toward a final solution of philosophical problems. Philosophy seeks to solve the most complex and difficult of human problems, and it is idle as yet to expect rapid progress in this field. We can then only be sure, by the use of rational induction, that it will no longer be necessary to give up ground that is gained, once we have securely established it. Revival and progress in philosophy are furthermore possible, but not sure, since the method of rational induction has demonstrated itself to be the only correct ultimate method of proof.

PART III

EPISTEMOLOGICAL AND METAPHYSICAL IMPLICATIONS

CHAPTER XI

THE USE OF RATIONAL INDUCTION IN SCIENCE AND LIFE

§ 1. A science, in the proper unrestricted sense of the word, is any body of knowledge that has been reduced to law and embodied in system, including those pieces of near-knowledge that may become knowledge. It is a sad commentary upon the popular estimation of philosophy that, in the English usage of the term, philosophy has ceased to be considered as a science, and the term "science" has been popularly restricted to those bodies of knowledge which have used the empirical and inductive method. While philosophy has been priding itself upon possessing the only certain knowledge, thinking that the natural sciences have attained only to probability and not to the highest certainty, philosophy has ceased to be popularly thought of as a science! Perhaps the philosophic depreciation of the natural sciences has been a compensatory reaction, and, with the adoption of the rational inductive method by philosophy as well as by natural science, philosophy, too, will come to be considered a science, as it rightfully should.

As an exposition of the method of science, this book might well have closed with the preceding chapter. But there are some epistemological[1] matters that still need discussion, and a few metaphysical problems whose solution becomes easy by the use of the inductive method; these are discussed in the remainder of the book. The subsequent discussion should not, however, be regarded as an integral part of the logical theory of rational induction, and a mistake in it should not be considered to militate against that theory.

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[1] The term "epistemology" is used in this book in the sense which it has gained in modern treatments, as the discussion of the *nature* of knowledge; whereas the wider logical problem as to the standard by which to criticize aspirants to knowledge is called the "problem of knowledge." These two problems, the method of criticizing knowledge and the nature of knowledge, should be distinguished.

The first problem that needs discussion, one that we shall find leads us directly into epistemology, is the problem of the scope of rational induction. Where is rational induction used?

§ 2. The perceptual judgment has presented a serious problem for logic. A perceptual judgment purports to be an account of objects that are present to the senses, or of the relations between those objects; for example, "This is a rectangular table," "That is an armchair," "The door opens to the right," etc. Such perceptual judgments should be distinguished from what we have called immediate descriptions.[1] The immediate description is an account of my own conscious experience; the perceptual judgment is an account of objects and their relations. The table rarely appears rectangular to me, although I regularly refer to it as rectangular, and would find it very difficult to describe its appearance in exact terms. Immediate descriptions are so uncommon in some fields (especially in reference to objects) that the very words necessary to make such a description are lacking. Try to give an exact immediate description of the appearance of such an object as a chair, and see how few things about it can be stated exactly. "Legs," "back," "rounds," etc., are, of course, interpretations, constructions, complexes of past and present experiences, and cannot be used in an immediate description! Geometrical terms are lacking for the description of even a straight chair when seen in perspective. A chair may be drawn, but hardly described.

The object, of which I usually give an account, is different from my experience. I may say that I perceive a four-legged wooden chair with a slightly sloping back and a seat which is slightly tilted backward and hollowed out. Such a perceptual description[1] of an object includes a great deal that is not actually experienced now. A leg is more than a series of colored points arranged in approximately the shape of two elongated trapezoids having two different colors, one wider and lighter, the other narrower and darker—as the leg appears to me. The term "leg" stands for sensations of touch as well as of sight; it is, moreover, thought to support the seat of the chair—a relation which is not given in experience, but constructed out of various elements, some of them quite remote from the thing I am observing.[2] Upon consideration, I find that I do not strictly perceive the back as slightly sloping with reference to the seat, nor the seat as tilted, but the top of the back of the chair is farther from me than the front, and the upper surface of the front of the seat is a little nearer to me than it should be if it were flat. In these and many other ways, the chair of which I give a perceptual description is different from the experience that I have. Yet I say that my experience is *of* the chair which I describe. Wherein is the connection, and by what logical means can I establish that connection?

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[1] Cf. chap. vii, § 4.

A little consideration will show that such a perceptual description of an object is logically a hypothesis, which is verified by deducing from it, in accordance with the laws of perspective, etc., the experience that I have. A leg has four sides; in perspective I can see only two; one will be slightly in the shadow. Thus from a four-sided leg I can deduce its two-sided appearance. Likewise from the hypothesis of a tilted seat I can deduce the phenomenon of the front end being a little nearer my eyes than usual, in comparison with the rear of the seat. So with the other perceived appearances: each of them is deducible, upon a little thought, from the hypothesis of an object which is more than what I actually perceive.

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[1] A "perceptual description" and an "immediate description" are different.

[2] Cf. chap. v, § 4, for an example of the construction of a relation.

Such a hypothesis is not often completely verified. My experience with chairs is so ample that I have learned what characteristics are to be expected of a chair; since I rarely find experiences which are at variance with deductions from my hypothesis, I do not usually take the trouble to verify a perceptual hypothesis completely. But if I should perceive something unusual about the chair, i.e., if the hypothesis of a chair were found not to deduce, in some respect or other, the appearance that I have, then I would very likely proceed to examine the object, i.e., to try to gain a wider experience of it, for the purpose of verifying or disproving my perceptual hypothesis that it is a chair. I would not only seek for visual sensations, but also tactual sensations and possibly auditory or other sensations; the definition (i.e., hypothesis) of a chair predicts certain things—legs or a pedestal, and a back—which predictions I might verify by seeking the experiences deducible therefrom; I might seek to find the four sides of a leg, etc. The exigencies of practical life make such a complete verification of a perceptual hypothesis rare; I only go through the long and complex process of complete verification when my account is challenged by another person or when some appearance of the object challenges my conception of it. If my hypothesis is not verified, I may seek a different one—the body of essential definitions, together with those definitions that approximate essentiality, constitute a collection of hypotheses from which I select one that fits (i.e., deduces the appearances of) my object. The scientific definition, when of a "natural object," such as a chair, horse, valley, etc., is pre-eminently such a possible hypothesis.

We can now see why immediate descriptions of our experience of objects are so rare in comparison with perceptual descriptions of the objects themselves. Immediate descriptions are not necessary; from the perceptual description as a hypothesis can be deduced directly the experiences that we have, and our hypothesis can be verified without taking the time and pains to give an exact immediate description. Furthermore, the immediate description of my experience is individual; no one else may see the object exactly as I do; whereas a perceptual description enables me to deduce a host of appearances of various sorts. Even when two people cannot have the same experience of an object, because of different perspectives, color-blindness, or different interests or prejudices, those different experiences may be deduced from the same hypothesis, and the object recognized to be identical. Thus the perceptual description is very much more convenient for actual use than the immediate description. Its convenience and simplicity make it so useful that oftentimes the very words necessary to give an exact immediate description are lacking. On the other hand, in the field of emotional experiences, where the emotion is always individual and rarely shared by another person, direct descriptions are more common.

Another reason that the adult does not spend much time in verifying perceptual hypotheses is that as a child he very likely spent a good deal of time revising and verifying hypotheses and in learning what sorts of experiences may be expected to be derived from different sorts of objects. While we must be careful not to assert too much reasoning power of children, yet it is true that even very young children do make inferences, and are capable of making deductions, which are sometimes surprisingly keen. They have found in the past that certain experiences go together, and, given the one, the other comes to their mind as an association, which is logically a deduction from an assumed hypothesis. Adults do not often trouble to gain tactual sensations of objects, although perceptual descriptions are usually in tactual terms—but children are very active in getting tactual experiences of many things, even seeking gustatory experience when not prevented—so that an adult has already made many connections between tactual and visual appearances, and is able to deduce the one from the other. An invariable connection between two experiences means the establishment of an empirical law connecting them, and the psychological association becomes a term in a logical connection.

This account of perceptual judgments as hypotheses enables us to distinguish between a true account and an illusion. An illusion is a hypothesis that cannot be verified. In the Müller-Lyer illusion, the two lines are *seen* to be unequal; but the hypothesis that they are unequal cannot be verified; I take the evidence of length given by the ruler as correct, because in the past I have found it to check with a wide range of experience, and so I treat the apparently unequal length of the lines as an apparent exception to the hypothesis; my decision that the exception is only an apparent one is confirmed when psychological investigation shows that my eye actually traverses a shorter distance on the one segment of the line than on the other, and the apparent shortness of one line accordingly becomes deducible from the hypothesis that the lines are equal—the apparent exception is taken up into the hypothesis and becomes a verification of it.[1]

When any object is repeatedly experienced, the appearances of the object, which the child laboriously distinguished or confusedly associated, fade into the fringe of consciousness and come to "mean" the concept, or perceptual hypothesis, so that the actual appearances do not often come to consciousness at all; we perceive the table as rectangular and level, although its appearances are neither rectangular nor level. Any process, even the logical verification of hypotheses, if often repeated with the same results, becomes unconscious and habitual. Consequently, the adult often is conscious only of the percept or of the conclusion of his reasoning, and not of the appearance of the objects or the steps in his reasoning. Since habit makes very exact the adaptation of percepts to the appearances of objects, and failures are uncommon, the adult comes to take a very few sense data as meaning many others; so that percepts alone are perceived. To the huntsman, a minute speck in the sky becomes an approaching bird; to a friend, a confused mumble becomes a clearly spoken word or phrase; others, getting the same sensation, fail to perceive the bird or hear the word. The logical process is swallowed up in the psychological; it only emerges when the mechanism of action ceases to act smoothly, and reflection again becomes necessary.

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[1] When this account of perceptual judgments or perceptual descriptions as hypotheses had already shaped itself in my mind, I found that I had been anticipated by R. H. Dotterer, in *Beginners' Logic*, pp. 142 f.

It is a remarkable fact that we often perceive objects in terms of our hypotheses or conceptions of them. Somehow the conception is superadded to the sensations and the two can only be distinguished by considerable reflection and analysis. Only the artist or sophisticated person who has learned to deal with the apparent shapes of objects perceives the table as other than rectangular, although it can rarely appear as such. The perception of depth in binocular vision is a most remarkable case of this phenomenon. Two different plane images of the same object are received by the eyes, yet what is perceived is not two images, but one object with the added quality of depth. The stereoscope illustrates this fact admirably. Instead of two different images, we perceive a construct from which can be deduced both images—a figure with depth. So unconscious has this interpretation of binocular vision become that few human beings can perceive the two different images, even where there are decided differences between them, except by closing one eye, thus forcibly isolating them. It is not that the mind unconsciously performs a logical inductive and deductive inference; such an account would be mythology: our conception of the object as containing depth, etc., has verified itself so often in experience, so that, by a process of short-circuiting, we come to think of the conception directly, when we have one of the experiences which go into the making of it, instead of the experience itself. It is only when challenged that we reflect and find the logical basis for our sureness that our perceptual judgment is right.

It need not be supposed that this psychological phenomenon of experiencing the interpretation together with the sensation gives any trouble to logic. Were we compelled to accept all perceptual judgments at their face value, we should be helpless before the fact of illusion. But the procedure of taking the perceptual judgment as a hypothesis and testing it as other rational hypotheses are tested enables us to deal with illusions and all other such facts of experience. Immediate description gives an impartial account of whatever is experienced, whether it is a perceived interpretation, illusion, or correct perception. By taking the conception of the perceived object as a hypothesis, we can deduce its various appearances, both its perceived rectangularity and its apparently trapezoidal shape. Deduction can make allowance for the influence of interpretations or prejudices in the molding of experiences, by including their causal circumstances in the hypothesis, and deduce from them the actual experience, of whatever sort it may be. In this way we may verify the presence or absence of interpretation or prejudice and the true nature of the object when these are removed. Only rational induction, in its full complexity, can deal with the perceptual judgment.

We must not, however, conclude that, since perceptual judgments are logical hypothesis, almost all thought becomes inference. We must remember that most percepts are not verified, for the reason that the habitual correctness of perception makes it unnecessary. Logical inference only occurs where reflection and criticism are found. Furthermore, we must remember that the process of discovering hypotheses or forming percepts is not a logical process, and that logic can only begin its criticism when the hypothesis has already been discovered and stated. Consequently, perceptual judgments only rarely become logical processes in practical life. It is, however, important that there should be the possibility of subjecting each and all to logical criticism, since, together with the indubitability of the experience upon which they are founded, this logical character of perceptual judgments gives to science the possibility of absolute certainty.

§ 3. The perceptual judgment offers great difficulty to the traditional logic, which finds knowledge only in deduction, i.e., where two propositions are combined to infer a third; for perceptual judgments are based directly upon experience, and deductive processes cannot deal with experiences directly, but only with conceptual material. The position into which one of the most eminent logicians was accordingly forced is very illuminating.

> In those judgments of perception . . . . there is properly no *datum*. . . . . The basis from which they start, and on which they act, is *for the intellect* nothing. . . . . We needs must begin our voyage of reasoning by working on something which is felt and not thought. The alteration of this original material, which makes it first an object for the intellect, is thus not yet inference. . . . . We cannot take it as the starting point of inference. . . . . Thus our primitive judgment falls short of inference in two main points. It is doubtful first (i) if the operation performed is not purely capricious. . . . . We have no rational ground we can produce, in order to justify our result. . . . . Secondly (ii) the stuff, upon which the act is directed, is not intellectual.[1]

[1] Bradley, *Principles of Logic*, III, I, vi, 15, p. 440.

What a terrible situation—the material with which inference begins is not original but derived, and shows marks of intellectual activity; yet that derivation cannot be criticized by logic, but nevertheless must be accepted—as a possibly capricious or deceptive act! Yet if deductive logic is the only possible form of logic, such a conclusion is inevitable. Deduction can only deal with propositions, whereas the perceptual judgment deals with experiences. Since the logical criticism of perceptual judgments cannot be a deductive matter, it can only be non-logical! Deductive logic cannot even get a *ποῦ στῶ* upon which to begin inference.

It was the failure of the ancient world to recognize induction as a logical process, and perceptions as really implicit inductions, that led the ancient logicians to doubt the truth of sense-experience and to an ultimate skepticism. They recognized the relativity of sense-perception, and the syllogism as the only logical instrument; then, finding the two incompatible, they concluded that truth is impossible. Such ought also to have been the conclusion of modern thinkers, but the success of natural science has prevented skepticism from becoming popular.

The inevitable difficulties of any logical theory that fails to permit a logical criticism of perceptual judgments led to other suggestions. For Mill "that which it is a conception of is really *in* the facts, and might . . . . have been detected."[1] The rectangularity of a table none of whose angles are seen as rectangles is *in* the facts! Of course, the perceptual construction of the appearances as *meaning* a rectangular table is in the mind; but the question Mill ought to have faced is, What right does a non-rectangular appearance have to *mean* rectangularity?

In a similar vein, Bertrand Russell writes:

> In every philosophical problem, our investigation starts from what may be called "data," by which I mean matters of common knowledge,

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[1] *System of Logic*, Bk. IV, chap. I, sec. 2, p. 426.

vague, complex, inexact, as common knowledge always is, but yet somehow commanding our assent as on the whole and in some interpretation pretty certainly true."[1]

Notice all that he is compelled to say about these 'data'—they are knowledge, i.e., propositions; they command our assent; and are somehow true—but yet they are the *starting-point* for logic! If so much truth that commands assent can be obtained without logic, why cultivate logic? If a table whose angles are rarely seen as right angles nevertheless "somehow commands our assent" as being right-angled, and if philosophic criticism must *start* with a right-angled table, surely the attempt to criticize all knowledge cannot go very far! Or again, "The immediate facts perceived by sight or touch or hearing do not need to be proved by argument, but are completely self-evident.[2] But when, with the Muller-Lyer illusion, two lines are seen as unequal and are measured as equal, is both the inequality and the equality of those lines self-evident? If we must be thus uncritical in accepting the conclusions of "common knowledge," why not also accept the common uncritical opinion that objects exist external to minds or that universals are mere names?

To say that anything is a "fact" is to mean that it is true. Since perceptual judgments give the "facts" of common sense, logic must be able to test these "facts" as well as other "facts." If the senses never deluded anyone, the problem of logically testing the validity of perceptual judgments might not be so important, but the existence of error and illusion makes it imperative that perceptual judgments be logically criticized as well as other reasonings. The one-sidedness of any logical theory that does not admit hypothetical induction as a logical process and consequently does not provide for a logical criticism of perceptual judgments is shown by the difficulties into which it falls.

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[1] *Scientific Method*, p. 65.

[2] *Ibid.*, p. 68.

§ 4. The fact that perceptual judgments are logically tested and validated by the hypothetical method aids us in showing that this method does give certainty. There are few judgments which will be more universally or more quickly agreed upon than such perceptual judgments as "This is a chair" or "That is a book." Yet the characteristic of being a chair or a book is not given in the sense-data, but is a construction from those data—a hypothesis. Such judgments are not invariably correct; people make mistakes even about chairs and books. Yet upon a little consideration or manipulation of the object, anyone will concede the truth of such a perceptual judgment. As Mr. Russell says, they "somehow command our assent as on the whole and in some interpretation pretty certainly true."[1] These perceptual judgments are, moreover, validated by a process of hypothetical inference and by the application of the same criteria as other hypotheses—wide range of verified deduction. Then hypothetical inference must give certainty, for perceptual judgments are surely certain, at least in most cases.

Or shall we doubt the certainty of perceptual judgments too? The traditional logic cannot accept the perceptual judgment as a verified hypothesis, for that would contradict the traditional dogma that induction can give only probability—that it is only probable that this is a book which I am reading, and I can never tell with surety what is a book and what is not a book! No matter how clear and exact a definition of a book I make, and how exactly I state my perception, I cannot know that I have a book! Even if under the most diverse circumstances the object showed those characteristics which are deducible from the conception of a book, and if my friends and

[1] *Ibid.*, p. 65.

enemies alike agreed in testifying to the correctness of my hypothesis that it is a book, even yet I can have only probable knowledge that I am right! And yet the perceptual judgment is admitted by logicians to be the foundation of reasoning. There could hardly be any surer prelude to absolute skepticism than this doubt! Either we must agree that the traditional logic is wrong, and that induction can and often does give certainty, or else we are thrown into a skepticism from which nothing can rescue us.

§ 5. If perceptual judgments are hypothetical inductions, is it possible to gain a sure and indubitable foundation for all knowledge? Yes, it is possible. Descartes, in his *Cogito ergo sum*, discovered such a foundation. His saying must not be interpreted in the way that he took it, as establishing the existence of a soul, but in the sense, "Thinking exists, therefore there is thought," that is, the only thing which cannot be doubted is experience, because, when an attempt is made to doubt the existence of experience, the doubt itself is nevertheless experienced. This experience, which contradicts its own doubt, is unique in that there is no other *experience* which establishes itself upon denial. Though we can no longer accept Augustine's *Quod si fallor, sum* or Descartes' *Cogito, ergo sum* in the sense that they interpreted it, it nevertheless does represent a profound discovery—that the one thing which is indubitable is experience itself. If I have a vision of a ghost, I cannot doubt that I am having the experience, though I may well doubt whether there is really any ghost there to be seen. A ghost is just as much a part of my experience as a chair. Experience is, however, neither true nor false—it is simply experience. It only becomes true or false when it is translated into propositions. The unshakable foundation of all knowledge is experience, which includes everything that passes through a mind—illusions, emotions, reflections, doubts, as well as correct perceptions and inferences. Upon this foundation is built all knowledge: immediate descriptions, by stating in exact conceptual terms the contents of experience; perceptual judgments, by building hypotheses, deducing their consequences and verifying them; other judgments, by further hypothetical or deductive inference.

§ 6. The recognition of verified perceptual judgments as the products of a logical process leads us to a conception of a hierarchy in the orders of knowledge. The solid substratum upon which the hierarchy rests is experience. Experience is neither true nor false, not knowledge, just data. The first order of the hierarchy is composed of immediate descriptions. These are derived from experience as directly verifiable hypotheses without the use of deduction. In many cases this first order is neglected, and we mount directly to the second order, that of perceptual judgments. These are the lowest of deductive hypotheses, and in some cases differ considerably from the appearance of the object. Above these two orders there are other orders of knowledge, which may be variously specified. The two most important for logical purposes are the empirical law and the rational inductive law, the latter with various degrees of more and less limited universals.

It should be observed that the figure of a hierarchy is in some respects misleading, in that the higher orders of knowledge do not necessarily depend upon the lower, but that from a hypothesis, of no matter what generality, deduction may be made directly to experience, and the hypothesis verified or disproved without any more than an implied reference to immediate descriptions or perceptual judgments. Newton deduced from his law of gravitation directly to the predicted experience of the apparent positions of the spots of light which were interpreted as planets, as well as to Kepler's laws. This remarkable property of hypothetical induction is due to the power of deduction, which enables a hypothesis to be translated into statements of any order at will.

What, then, are meant by "facts"? The term "fact" has come to be used so loosely that it is synonymous with knowledge. The appearance which I interpret as a ghost may be called a "fact" (an immediate description as a "fact") or the law of gravitation (a rational induction of a very high order) may be taken as a "fact." Usually, however, a "fact" is a piece of knowledge of an order lower than the one that I am considering, upon which I base my present judgment, either by induction or by deduction. Thus a "fact" becomes any piece of knowledge which is an inductive or deductive ground for the one I am considering. For Newton, Kepler's laws were facts, whereas for Kepler, Tycho's observations (immediate descriptions) were facts. The term "fact" is usually used relatively to the matter under consideration.

§ 7. Mr. Santayana has shown that skepticism may ground itself, not merely in the doubt of the perceptual judgment, but also in the claims of memory to give a correct picture of the past.[1] Even at best, he says, an immediate description can only be an account of a specious present; the actual present can never be observed as it is passing; hence the most immediate perceptual judgment assumes the trustworthiness of memory. History and practical life presume upon the trustworthiness of memory even more. The traditional logic cannot establish the trustworthiness of memory any more than the correctness of a perceptual judgment; hence skepticism is unavoidable. Eloquent as is Mr. Santayana's account, one cannot help feeling that his skepticism is not "criticism surprising the soul in the arms of convention," but an inadequate and partial logical method, uncritically accepted, failing to validate the knowledge that childhood had secured by a more adequate method.

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[1] G. Santayana, *Scepticism and Animal Faith*, chap. iii.

The deductive method fails to justify either the correctness of perceptual judgments or the trustworthiness of memory, and, if trust is put in that method alone, such skepticism as Mr. Santayana's is inevitable—he is merely carrying to its logical conclusion what is implied in the traditional logic but what few have had the courage to confess—that, since certain and ultimate premises are impossible, no knowledge is possible at all. Nevertheless, the inductive method enables us to criticize and validate the trustworthiness of memory just as it enables us to deal with perceptual judgments.

How is the trustworthiness of memory proved by inductive logic? In the same way that we naturally set to work to prove the trustworthiness of any remembered event—by taking it as a hypothesis, deducing as different sorts of consequences as possible, and checking them with experience, using the main criterion for the verification of a hypothesis and as many of the supplementary ones as fit the case. I remember that last night I went to a university class. If I am very much concerned to verify my memory, I deduce from that hypothesis that so-and-so whom I saw there should also remember the event, so I communicate with him. I look for the notes I took of the lecture, or see whether there is as much gasoline in my car as there was yesterday at supper-time; I may even go to the room and see whether the chairs are still disarranged or find if the janitor has any memory of rearranging them—then, by a wide variety of consilient inductions, I reach the conclusion that my memory is correct. A very good example of the lengths to which this testing and corroboration of a remembered event may need to go is found in the verification of the confession of a criminal. The law courts have in many cases put into practical operation the principles of rational induction, since the human importance of the matters at issue has compelled them to seek the utmost in certainty.

The difficulty and time-consuming nature of such an inductive verification make such logical testings rare in practical life; for we have learned that accuracy of memory is a general quality, and that, if memory is accurate in one matter, it is usually also accurate in other matters. So we frequently use matters that can be easily tested to make a general judgment of the accuracy of a person's memory, and discount or esteem his testimony accordingly. In this way we are taking a risk, whereas by carefully verifying every incident in a memory we take no risk; but in most cases the risk is very small, and a general test saves time and effort tremendously. In other matters we are more careful. Thus we find no difficulty in justifying our belief that Julius Caesar once lived; not only is there more than one chain of personal and documentary testimony from that time to this, which has been criticized and found to be more than moderately reliable, but there are other historical events, such as the downfall of the Roman Republic, equally well documented, which can only be accounted for on our belief. It is the consilience of these widely different chains of evidence that establishes the certainty of such a historical fact.

§ 8. The way to escape skepticism is now open. As a first foundation, we have the indubitabilities of experience. As a means we have the self-proving method of rational induction. As results we have the possibility of testing and critically establishing or rejecting any perceptual judgment or memory. Thus we are able to escape all the more fundamental attacks of skepticism, and should not find it difficult to attain to knowledge of those things which are commonly regarded as true, a knowledge which will be certain in the sense that it has been logically tested and verified.

If it is objected that the proof of the method of induction implies the correctness of memory and of perceptual judgments, and that each of the others requires the other two in order to establish itself, so that there is a *petitio principii* in thus founding each on the other, we reply that *petitio* is a purely deductive fallacy, and that in the proof for each of these three foundations of knowledge the assumption of the other two is not made in such a way as to prejudice the issue. It is quite possible that, assuming the correctness of the inductive method and the proof of perceptual judgments, memory should prove false—in fact it does often so prove—and that likewise, in the case of each of the others, the assumption of the truth of the other two is quite compatible with the falsity of the one which is being considered, and our logical method would show that falsity, if it exists. Hence no fallacy is committed, and our proof remains firm. Philosophical skepticism has lost the first and most important battle; its ultimate rout is in sight.

§ 9. The scope of the inductive method in life is very large. It may be used to test the truth of any judgment of fact, except such as are deduced or merely assumed—that is, it may be used in testing almost any supposed facts. Such propositions as "My home is east of the school," or "My friend is worthy of trust," or "John is the brother of James," are tested by taking them as hypotheses, deducing a wide range of consequences, and verifying them. If my home is east of the school, I should be able to face approximately in the direction of the rising sun in traveling from the school homeward, or, if buildings intervene, in directions the sum of whose components is east, etc.

Whenever we get into difficulties, we are pretty sure to try the inductive method in our endeavors to get out—to take one alternative after another, go far enough in the process of verification to see whether it will lead us to a correct solution or not, and, if not, try another alternative. Logically considered, the method of trial and error is the use of a succession of hypotheses chosen at random. The characteristic which distinguishes it from so-called rational thought is not that rational thought is logical where the other is irrational, but that rational thought selects its hypotheses before trial by a process of conceptual deduction and imaginary trial instead of using mere chance selection and brute experimentation with the situation. Either method might logically establish or disprove a proposition; the method of rational thought reaches the goal quicker because it eliminates false hypotheses before objective trial and not only after it. The inductive method has extremely wide uses in practical life.

§ 10. In natural science, the wide use of the inductive method is well recognized. We have already given illustrations from some of the sciences.[1] Analysis and classification, two methods used widely in science, are likewise justified by the inductive method.[2] Since the verification of an immediate description (which is the same in nature as the procedure followed in testing an analysis) is much easier than that of a rational hypothesis, the correctness of an analysis is proved much more easily than that of rational induction. It is consequently not surprising that analysis should have been used widely in philosophical discussion as well as in natural science.

§ 11. A fact of practically greater importance is that the inductive method is used in dealing with plans of action and valuations. The peculiar thing about these judgments is that, while they are taken as hypotheses, and deductions are made from them, the correctness of the deductions and of the hypothesis is not verified until after the decision has been made and the hypothesis adopted as correct. For example, I judge that it will be a good thing for me to go to the theater; if

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[1] An admirable set of examples of the inductive method in various sciences is given in C. H. Patterson, *Problems in Logic*. Mr. Patterson groups his cases under Mill's methods, but analysis will show the use of hypothesis and deduction in each case.

[2] Cf. chap. vii, § 4.

I stop and reflect upon the correctness of that judgment, I will probably support it by the deductions that I shall enjoy the play, or I shall see so-and-so, hence I conclude that, since these consequences of my act will be pleasurable, I should go. Until I have actually made my decision and have gone to the theater, I cannot be sure that my deductions were correct or sufficiently comprehensive. I may find that my deduction was incorrect—that the play is not enjoyable; or I may find that there are other deductions I ought to have made but did not—that I am unprepared for class the next morning, and that consequently my action was bad rather than good. All ethical judgments of value suffer from this handicap—they cannot be confirmed before a decision has to be made. The same is true of any experiment, practical or scientific; the correctness of the predicted result is not known until the experiment is made. In science the making or non-making of the experiment is not such a momentous thing as in practical life. The difference between the practical judgment and the theoretical judgment of science is not in logical method; but that in practical life we have to decide the matter *before* we act and can verify our judgment, whereas in science we do not decide until after verification. Hence faith is much more important in practical life than in theoretical disciplines.

"Good judgment" in the conduct of life would thus seem to be an accumulation of principles of deduction—a knowledge of what are likely to be the results of various courses of action, combined with an insight into the nature of different situations, so as to be able to tell which category of deduction to apply to any particular case; together with an agile mind that makes *all* the pertinent deductions from a particular hypothesis and omits none of importance. Disappointment comes as often from the occurrence of an unexpected consequence to a plan of action as from wrong predictions. Therein lies the secret of

Aristotle's confession that the practical part of ethics cannot be a science—he could not apply his deductive syllogistic method to it, since verification is by induction only! Knowledge of the probable consequences of actions comes from experience; human life is so complex and its situations so difficult of analysis that there is as yet no science of the conduct of life, though precepts and good advice are plentiful.

It is a deplorable fact that the conduct of life should have been to such a large extent left to the mercy of moral superstitions and the hasty generalizations of unscientific minds, instead of being put on a secure basis. In spite of its extreme difficulty, it is to be hoped that a science of the conduct of life will be worked out. A beginning is being made in sociological and economic fields, but in the more intimate fields of personal contacts, little has been done outside of exhortation.

What, then, is the logical function of ethical precepts? We have seen that the suggestion of a hypothesis that will solve a problem is often the most difficult of all the steps in its solution. Ethical and practical precepts are suggested hypotheses. The precept, "Return good for evil," is a hypothesis, which may be applied to pertinent cases of conflict; while it is in opposition to some human instincts, yet in a large proportion of the cases in which it has been used the results have been extraordinarily successful, although there are some cases in which it has failed, or in which its failure has been brought about by extraneous factors. It should be the business of scientific ethics to search for the conditions of failure and success of such a hypothesis as this. Just as it took a scientific genius to think out first the law of gravitation or the laws of electromagnetics, so it took moral geniuses to discover these hypotheses of moral action, and to dare to put them into practice for the rest of the world to witness the consequences of their adoption. That he who dared to preach and practice "Love your enemies" was put to death shows the heights of heroism required of the moral innovator; by the fruits of his life, the value of that type of action can be truly esteemed. Such moral precepts are invaluable hypotheses for practical life, especially when it has been shown what consequences should be deduced from them.

The scope in which the inductive method is used is thus extremely wide. Perceptual judgments, the trustworthiness of memory, practical judgments of fact, scientific discoveries, and plans of action are all susceptible of validation only by the inductive method; by its use we find that a secure foundation is laid for knowledge, and the claims of skepticism are refuted.

§ 12. It is time now to ask the question, Can the inductive method be used in philosophy? We may reply that we *have* been using it in philosophy and that is sufficient; but such a reply, while conclusive, may not be persuasive. The obscurity which has veiled the nature of the inductive method has brought about the popularity of the view that each science has its own method, and that these methods have little or nothing in common. We hear of the legal method, the historical method, the statistical method, physical method, biological method, etc. Philosophy, in particular, has often been thought to have her peculiar method, distinct from the scientific method or methods, and the method of philosophy has been thought to give her a peculiar certainty that distinguishes philosophy from the special sciences, which can attain only to probability.

Why should it not be possible for philosophy to use the same method as the other sciences? Are these disciplines so different from each other that nothing is common to them, not even a method? Methods of investigation and discovery vary—but do methods of proof differ? If our analysis is correct, they do not. The historian of science, drawing from his knowledge, reports, "We are led to believe that the nature of truth is in all subjects the same, and that its discovery involves, in all cases, the like condition."[1] Huxley has put the matter very illuminatingly:

> Science is, I believe, nothing but *trained and organized common sense*, differing from the latter only as a veteran may differ from a raw recruit: and its methods differ from those of common sense only so far as the guardsman's cut and thrust differ from the manner in which a savage wields his club. The primary power is the same in each case, and perhaps the untutored savage has the more brawny arm of the two. The *real* advantage lies in the point and polish of the swordsman's weapon; in the trained eye quick to spy out the weakness of the adversary; in the ready hand prompt to follow it in the instant. But, after all, the sword exercise is only the hewing and poling of the clubman developed and perfected. . . . . A detective policeman discovers a burglar from the marks made by his shoe, by a mental process identical with that by which Cuvier restored the extinct animals of Montmartre from fragments of their bones. Nor does that process of induction and deduction by which a lady, finding a stain of a peculiar kind upon her dress, concludes that somebody has upset the inkstand thereon, differ in any way, in kind, from that by which Adams and Leverrier discovered a new planet. The man of science, in fact, simply uses with scrupulous exactness, the methods which we all, habitually and at every moment, use carelessly. . . . . If, however, there be no real difference between the methods of science and those of common life, it would seem, on the face of the matter, highly improbable that there should be any difference between the methods of the different sciences.[2]

If the method of the natural sciences is an outgrowth of common sense, is philosophy anything different? Does philosophy deal with problems that are entirely unrelated to ordinary life? No, philosophy is an outgrowth of natural and inevitable human questionings just as are the special sciences.

Or is it that philosophy deals with a transcendent or transcendental world with which the special sciences cannot deal? To do that, philosophy would need an absolutely secure method that would be apodictic. Here probability would spell defeat. Has philosophy justified itself in its claim to deal with such a

[1] W. Whewell, *Philosophy of the Inductive Sciences*, Vol. I, p. 3.

[2] Huxley, *Lay Sermons, etc.*, pp. 77 f.

realm by securing the unanimity that comes from a secure method of proof? Since the time of Kant, philosophers have been too humble to claim transcendent knowledge; more and more they have been dubious of transcendental knowledge. Philosophy has restricted itself to the same world as that with which common sense deals.

Or is it that philosophy deals with more fundamental or more comprehensive problems than do the special sciences? If so, only a difference in kind of problem would justify a radical difference in method. A mere difference of degree would not justify such a difference in method. In truth, philosophy considers merely the broader and more comprehensive questions which have to do with the same world as that with which the special sciences deal.

Then is not this difference of degree so great as to constitute a difference in kind? Why so? In the ancient world the atomic theory was philosophy; today it is physics or chemistry. Historically, all the special sciences are the offspring of the mother-science, philosophy. Then how can they be different in kind? Where can we point to a definite boundary between the special sciences and philosophy? As soon as we deal with the fundamental problems of any science, we find ourselves led into philosophy. There is a perfect continuity between the fundamental problems of philosophy and those of the special sciences. Why, then, should there be a difference in logical method?

It is strange that this prejudice should exist, because the inductive method has been used in philosophy just as in the special sciences, though without the careful criticism which that method needs. We have seen that ever since the time of Kant,[1] the method of hypothetical construction has been a favorite one with philosophers, and that any philosophic system "appeals" because, when taken as a hypothesis, it permits the deduction of what one believes to be true. What philosophy has lacked has not been the use of the inductive method, but its critical and conscious use. What philosophy needs is not a different method, but the explicit and careful use of that method, together with a recognition of its extraordinary power. By its means, the new Copernican revolution in philosophy, which has been so widely sought, may yet be achieved.

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[1] Cf. chap. iii, § 19 ff.

§ 13. Before going farther into epistemology, we may well stop and ask ourselves, What sort of a theory of knowledge have we arrived at? What is the relation between knowledge and the experiences on which it is based? We have rejected the coherence theory; must we accept the correspondence theory of the nature of knowledge?

It is evident that the correspondence theory of knowledge is nearer the view we have come to than any other. Yet knowledge is not a relation between an idea and a thing of which it is the representation. Many of the things to which we refer, such as the sun and earth in an eclipse of the moon, do not appear in the experience which evidences them. We must reject the correspondence theory as not expressing exactly the relation involved.

Yet there is one sort of knowledge for which the correspondence theory is correct—knowledge that is reached by the process of immediate description only. When I give an introspective statement of my experience, there should be in my statement something which corresponds to every pertinent element in the experience, since I am attempting nothing more than an account of what I am experiencing. Here the correspondence theory of knowledge is quite correct.

That theory is true, however, of immediate description only; moreover, immediate description is of very limited value and has a restricted use. In the perceptual judgment as in the rational inductive law, there is no one-to-one correspondence between experience and knowledge. There is, however, an exact relationship—that the knowledge *explains* the experience correctly: that from the knowledge it is possible to deduce the experience; and that all pertinent experiences can be deduced *only* from this piece of knowledge. The relationship is thus deductive, but of a very peculiar sort—not mere deduction, but exclusive deduction. To use Descartes' famous illustration: the relationship between the view that the sun is 1,300,000 times the size of the earth and the small round bright spot we see is not one of correspondence; nor is it merely that, if we assume a sphere that size at a distance of 92,800,000 miles, by the laws of projection, it will appear the size it does appear; it is that when all pertinent phenomena are taken into consideration, the appearances of the other planets, their apparent motions, the apparent differences of direction they have when seen from different points on the earth, etc., as well as the apparent size of the sun—these phenomena can *only* be deduced from a hypothesis which includes that particular size of the sun. Such a deduction involves the principles of geometry and of mathematics as well as the laws of deductive logic. How immensely complex is the relationship between any piece of knowledge and the experience that it explains! How different it is from the one-to-one correspondence assumed by the correspondence theory! Only the simpler relationship of immediate description is one of correspondence.

According to this theory, then, there is no absolute gap between the world of eternal truth or knowledge, on the one hand, and the world of changing reality, on the other. Both reduce themselves to a common denominator, experience. By the power of deduction, the laws of nature combine and translate themselves into meanings which are applicable to the moment; by processes of causation, objects likewise translate themselves into the changing experiences of the moment: thus both realms of reality, ideas and objects, meet on the common plane of human experience. Meanings and sensations are equally experienced; conscious experience forms the bridge between the two realms. Yet far more than mere experience is needed to bridge the gap; while for immediate description that is sufficient, for the higher syntheses of the intellect there is needed a long chain of deduction, on the one hand, and a complex process of causation, on the other, before the two are reduced to a common denominator. Experience is only the middle link of a long chain which binds these two realms into one; it is the length of the chain and its exclusive character, rather than any other feature, that has prevented the full realization of its nature by philosophy. It is this inductive chain for which Socrates was seeking when he said, and yet doubted, that things participate in the ideas. Participation is too simple a term for the relationship; in this complex world of ours, the relation between knowledge and its objects is the extremely complex one of exclusive deduction.

CHAPTER XII

THE NATURE OF MATHEMATICS AND PROBABILITY

The proper activity, the distinctive function, of the mathematician is to mathematicize, as that of a swimmer is to swim; or that of a farmer to farm; or that of a poet, to make poetry; or that of a trader, to trade. And it is as little the business of the mathematician to define and evaluate the peculiar type of his proper activity as it is that of the swimmer or the farmer or the poet or the trader to do the like for his.[1]

§ 1. Upon the philosopher devolves the difficult task of determining the nature and proper functions of the special sciences. The fundamental principles of these sciences are his concern. Among the questions thereby raised, the problem of the nature and foundations of the sciences that are grouped under the name of mathematics has been so important in the history of philosophy and is such a searching test of the soundness and so revealing of the implications of a logical theory that no logical theory should be considered complete unless it succeeds in dealing with this important problem.

Our thesis has been that all knowledge ultimately comes by hypothetical induction and is based on experience, though it may not come from experience but may be a conceptual creation, which is verified by reference to experience. Mathematics certainly seems to be knowledge and to be more certain than any empirical fact, so that it cannot be based on experience. What, then, is mathematics and what is the nature of its validity?

Opinions of the nature of mathematics have varied from that of Mill, that it is a set of deductions from empirically established principles,[1] to the mystical view that "mathematics contemplates Being under the aspect of Eternity" and "is the study of Fate."[2] The opinion that perhaps has had the most currency is that of Kant, that mathematics is a priori, a formulation of the necessary laws of thought which are the inevitable conditions of experience. Kant found in mathematics a priori synthetic judgments, which necessitate the assumption of a transcendental faculty for their possibility. Even if we take the modern view that mathematics is merely a deductive development of what is implied in its fundamental axioms and postulates, and so view it as analytic, a pure creation of thought, nevertheless it is applied to concrete objects, which do not seem to be creations of thought, and it thereupon gives results which are seen to be true—applied mathematics is thus synthetic, in Kant's use of the term, and would seem to require a transcendental operation of the mind for its possibility. This transcendental mind would then condition experience so that all possible objects must conform to its laws of thought. Applied mathematics would therefore be the formulation of certain of these transcendental laws of mind, and be based solely on the laws of thought.

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[1] C. J. Keyser, *Mathematical Philosophy*, p. 27.

This argument is an illustration of the way in which any a priori theory of mathematics requires that the material of mathematics be unambiguous and universally unchangeable—so that it must consequently be impossible to think objects except in accordance with the accepted laws of mathematics. This view inevitably implies that Euclidean geometry is the only possible geometry and that such algebraic laws as the associative and commutative laws are inviolable. If the mind legislates for experience, it must legislate universally or else vacate its throne. The successful creation and use of the non-

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[1] *Logic*, Bk. II, chap. v, sec. 4, p. 172.

[2] Keyser, *Mathematical Philosophy*, pp. 135–36.

Euclidean geometries and of such algebraic devices as quaternions, in which the associative law is deliberately violated, show, however, that the legislation of mind is by no means fixed. The mathematical curiosities proposed by Huntington,[1] in which each one of the fundamental postulates of algebra is violated in turn, show that the flexibility of the human mind is seemingly unlimited, and that there is no way in which the world must necessarily be thought. Kant's assertion that the transcendental operations of the mind are necessary to explain the practical truth of mathematics bears the marks of an *ad hoc* hypothesis—it fails to afford other verifiable deductions in addition to the phenomena which it was invoked to explain, and no further phenomena have been brought forward by its upholders. Hegel's metaphysics may be considered as an attempt to produce such evidence by showing that the world is through and through the inevitable development of the nature of thought; but he failed to show any unambiguous course of such a development. Kant's hypothesis fails for want of a wide range of verified deductions, and so we must reject the conception of mathematics as based upon logically a priori principles.

How then shall we conceive of mathematics? Its first principles are surely not empirically established, for the mathematical point, line, etc., are not found in any but conceptual experience. Yet, in accordance with our theory, all knowledge is such only because it relates itself positively to experience. This relation is, however, through a deduction. There could then exist abstract systems of deduction, which are significant because of the possibility that they may be used for the elaboration of appropriate hypotheses. Such deductive systems would be neither true nor false in themselves, but merely self-consistent, true or false only when applied to concrete cases. That such is the nature of mathematics will appear in the sequel. If so, there will be two kinds of mathematics, which differ radically from the logical viewpoint, though their material may be largely identical: certain deductive systems, when considered merely as systems for possible use, and hence neither true nor false; and the same systems when used for the elaboration of successfully verified hypotheses, and hence true. We shall call these pure and applied mathematics, respectively.

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[1] *Monographs on Modern Mathematics*, J. W. Young, editor, chap. iv, and *Transactions of the American Mathematical Society* (1905), VI, 227 f.

In accordance with this view, pure mathematics would not be equally concerned with all possible deductive systems, but only with those for which there is likely to be concrete application. Mathematics, in its origin, is thus not unrelated to the world of experience, but is a selection out of a large number of possible deductive systems. Certain particular systems are developed because they are likely of application to concrete cases. Such laws as the associative law for addition, that $a+b=b+a$, are adopted, not because it is the nature of thought to think thus (a priori there seems to be no reason why $a+b$ should not $=-b+a$, and it was quite possible for Hamilton to conceive a system with the associative law stated thus, although it was a different system from that ordinarily used), but the reason that the associative law is stated as it is, is that in dealing with most countable objects their total number does not depend upon how they are grouped. Hence a mathematical system with the associative law as usually stated can be applied to most objects. Mathematics is likewise timeless because, for practical purposes, the time element is deliberately abstracted from. The foregoing account of mathematics will be established (in accordance with the principle of rational induction), if it can explain (enable the deduction of) the various characteristics of mathematics in its full modern development.

Before we undertake that task, we must make clear the precise nature of the distinction between pure and applied mathematics. Let us consider some examples. Euclidean geometry as it was conceived by Euclid, with concrete points, lines, planes, etc., as it is usually taught in elementary courses, is a branch of applied mathematics. Euclidean geometry as conceived by such modern mathematicians as Hilbert[1] or Veblen,[2] with its undefined indefinables, is a branch of pure mathematics. Algebra or calculus by itself belongs to pure mathematics, but when used for the solution of concrete problems it becomes applied mathematics. The difference consists in the relative degree of abstraction of the science. A set of primitive propositions, such as Hilbert's set for Euclidean geometry, that do not specify what are the entities to which they apply, but leave them undefined, may consequently be applied to various cases: not merely to ordinary geometry, but to projective geometry, number dyads, and an indefinite number of other cases.[3] In each case a different system is created, dealing with different entities, but isomorphic in that the form of the system is the same in all of them. Such an abstract formal system of deductions, which has been called a "doctrinal function," belongs to pure mathematics, while the various concrete[4] applications of it belong to applied mathematics.

§ 2. Pure mathematics is then the class of abstract and purely deductive[5] systems considered merely as following from sets of primitive propositions by the use of formal deduction, in particular, certain systems which are especially large and extraordinarily capable of application to diverse situations. The terms of such a deductive system and of its primitive propositions are left partly undefined for the sake of greater generality of application. In order to maintain the continuity with this science of pure mathematics, of what has been traditionally called mathematics, it would be well to restrict the term "mathematics" to those systems which afford at least one concrete application to the distinctively quantitative aspects of reality, and call those systems which do not admit of such an application by the name of logistic. It must be remembered, however, that pure mathematics does not deal with quantity as such, but consists merely in the abstract deductive development of sets of primitive propositions.

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[1] *The Foundations of Geometry.*

[2] *Monographs on Modern Mathematics*, chap. i.

[3] Cf. Keyser, *Mathematical Philosophy*, chaps. ii–vii.

[4] It must be remembered that the terms "abstract" and "concrete" are not absolute, but always relative to each other.

[5] It must be remembered that the mathematician uses intuition, induction, or any other non-deductive process in the *discovery* of his theorems—mathematics is by no means a deductive science in its origin—but pure mathematics is purely deductive in its *proof*. "Mathematical induction" is merely a particular sort of deduction.

It follows from this definition, in the first place, that, since the logical method of mathematical proof is deduction alone, pure mathematics cannot be either true or false. A theorem may be a valid or invalid deduction from the fundamental primitive propositions of the system, and so is sometimes called "true" or "false"—but such "truth" or "falsehood" is quite compatible with inconsistency between different systems. In one system, the sum of the angles of a triangle is equal to two right angles, in another system it is less than two right angles, and in a third it is more than two right angles—from the viewpoint of pure mathematics, all of these three statements are "true"—in fact when we consider the wide variety of possible mathematical systems, we see that there is no single theorem of pure mathematics which cannot be matched by an inconsistent theorem drawn from another system and just as "true" as the first! This kind of pure mathematical "truth" is merely the characteristic of belonging to a definite system, of consistency with the system, and is quite compatible with the widest disharmony with other systems which are a priori just as "true";

it has nothing to do with truth in the ordinary sense of the word. The theorems of pure mathematics should be called "valid" or "invalid" instead of "true" or "false." Truth or falsity belongs properly to applied mathematics and not to pure mathematics.

In the second place, since pure mathematics does not deal with truth or falsity, the primitive propositions of mathematics are entirely arbitrary. Pure mathematics may assume that there are an infinite number of lines through a point and parallel to a given line, or that such parallel lines are impossible, just as well as that there can be only one such line. It can assume at will an actual infinity or the nonexistence of an actual infinity. The character of the assumptions made will determine the character of the resulting system; but from the point of view of pure mathematics, one assumption is just as "true" as another. To date, there have been created a number of such alternative mathematical systems. The earliest and best known are the two non-Euclidean geometries, the hyperbolic geometry of Lobachevsky and the elliptic geometry of Riemann. Hilbert and Dehn have produced a non-Archimedian geometry by denying another postulate of Euclidean geometry. The denial of other postulates or combinations thereof has produced non-Desargueian geometry and non-Pascalian geometry.[1] Perhaps the most remarkable set of such incompatible mathematics is the twenty-nine different and independent algebras given by Huntington to prove the independence of his set of postulates.[2] Hamilton's quaternions and other hypernumbers represent an independent development in a different direction; the geometry of the fourth dimension and other hyperspaces is a generalization of ordinary threedimensional geometry. The Boolean algebra and other logistic systems represent a still different line of development. There seems to be little or no limit to the creation of such mathematical systems except the agility of the human mind and the consistency of systems. If pure mathematics, which includes all of these incompatible modern mathematical developments, were literally "true in all possible worlds," such a universe would certainly be a far more "higgledy-piggledy job lot of a world"[1] than the one that we live in!

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[1] Cf. Hilbert, *The Foundations of Geometry.*

[2] *Transactions of the American Mathematical Society* (1905), VI, 227 ff.; and *Monographs on Modern Mathematics*, pp. 192 ff.

Pure mathematics does not value these different systems equally highly. Most of them are mathematical curiosities, the playthings of a few speculative minds. Few of them possess a large set of deduced propositions and few form large systems. Furthermore, when a mathematician propounds a set of primitive propositions for an algebra or a geometry, he feels it to be his duty to produce an algebra or a geometry in which his postulates are fulfilled. What does this phenomenon mean? It means that a mathematical system is ultimately a means of deducing consequences from some hypothesis, for the purpose of applying the hypothesis to experience, and that the pure mathematician recognizes implicitly that even his most recondite deductive constructions are developments of that use.

The generality of which mathematicians are so proud in pure mathematical doctrinal functions is an added advantage for their use in the method of rational induction, since that generality permits the application of such a deductive system to a wider variety of hypotheses than if the terms of the system were given a determinate meaning. Mathematics has its twin foundations in playful curiosity and practical needs, and the latter one has been of greater importance than the former. If matter and energy prove to be discrete, as current physical theory seems to indicate, the physicist will demand the creation of a new mathematics, different from our present continuous mathematics, in that it deals with discrete quantities only; and such a mathematics will doubtless be forthcoming. While mathematics has not always developed out of practical problems, yet it is remarkable how many seemingly inapplicable mathematical developments (such as Gaussian co-ordinates) have been found to have a practical use. In mathematics, as in all theoretical sciences, while discovery does not wait upon practical use, yet those parts of the science which have practical application are developed earliest and to a larger extent than others. Few or no mathematical conceptions are entirely removed from the world of experience in which we live; all mathematical ideas are derived, by a process of greater or less abstraction, from the experience of human beings, and nearly always put together in ways suggested by human experience. As Sylvester said, "Most, if not all, of the great ideas of modern Mathematics have had their origin in observation."[1] Consequently, it is not surprising that they should have found their application in processes of reasoning about that world, and that they should be prized for the characteristics which make them widely applicable to that world. Pure mathematics consists of highly developed and very abstract (and therefore widely applicable) deductions from sets of assumed primitive propositions, which are consequently ready for use with any hypothesis that conforms to the characteristics of those abstract primitive propositions.

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[1] Bertrand Russell.

§ 3. The logical requirements which mathematicians have laid down for a set of primitive propositions are also those which make them suitable for use with a hypothesis. These requirements are self-consistency, independence, and sufficiency or categoricalness.

The self-consistency of a set of primitive propositions is established either by showing that any system deduced from these propositions must be isomorphic with a recognized mathematical system, whose self-consistency is known, or else by producing a concrete system that satisfies the primitive propositions and can be concretely represented, and therefore seen to be free from self-contradiction. The first method may be used with mathematical systems of which it is impossible to create a concrete representation; four-dimensional geometry is shown to be self-consistent when it is shown that an analytic analogue of it can be constructed. But this method cannot be ultimate, for the question arises, How do we know that the analogue is self-consistent? Consequently, the mathematician is reduced ultimately to the method of making a concrete representation of his system in order to insure its self-consistency. The assumption which underlies this process is that "no really existent system can have contradictory properties."[1] This is, of course, an inductive test. The indefinables in the primitive propositions are clothed with concrete meaning, so as to convert the primitive propositions into a concrete hypothesis, and to convert the system of pure mathematics into a system of applied mathematics; then the system is compared with a concrete representation, such as the system of points on a plane or of lines on a sphere, and, in this manner, shown to be capable of verification.

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[1] *Nature* (1869), I, 238.

This process of comparison shows that the requirement of self-consistency is for the sake of permitting the existence of concrete applications of the system,[2] and that it is ultimately an *inductive* property and not a merely deductive one. The consistency of one proposition with another cannot be deduced, it must be *seen*. Even when I have two propositions so expressed as to clearly contradict each other, such as "This A is B" and "This A is not B," I can only *see* that they disagree and that they fit into the general law of non-contradiction; for the general law is only an abstract form which says nothing about particular instances of it. It can only be *seen* that a case fits the general law by a process similar to that of immediate description; without such a process of seeing, the gap between the universal and the particular could never be bridged, and the fact of inconsistency could not be deduced.

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[1] Huntington, *Transactions of the American Mathematical Society* (1905), VI, 224.

[2] Cf. Keyser, *Mathematical Philosophy*, pp. 116–17.

It is furthermore evident that self-consistency in mathematics, in its ultimate meaning, means merely the possibility of concrete existence. The test of self-consistency is whether the members of a supposed contradiction can co-exist in a concrete representation; if they can, they do not contradict each other; if they cannot,[1] they are contradictory. By analogy or by the process of making a concrete representation of an abstract system, this meaning of self-consistency is extended to abstract systems.

The second requirement, that of mutual independence, means that no one of the primitive propositions can be deduced from any combination of the others. For the sake of simplicity, it is necessary that in a statement of primitive propositions there should be no redundancy or overlapping of one assumption upon another. In itself, such an overlapping would do no harm, provided the propositions are consistent with each other; but it would be a violation of the mathematician's conception of elegance. This mathematical requirement of elegance grows out of the demand of the human mind for such simple statements as can be easily comprehended and readily dealt with, and therefore is worth while. It is a purely heuristic principle, itself not logically indispensable, though practically valuable.

At present, the only method of proving the independence of each set of primitive propositions is to produce a mathematical system that obeys all the other postulates of the system except this one. While this is a correct method, it requires an extraordinary amount of mental agility, intrinsically of a peculiarly useless sort, to invent the systems required. It is a curious commentary upon the essentially inductive character of critical thinking that the mathematician, who deals with a science fundamentally deductive in character, should instinctively resort to such a non-deductive method in dealing with the fundamentals of his science!

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[1] In the meaning of "possibility" we have given, cf. chap. viii. § 8.

The failure of the mathematician to recognize the fundamentally syllogistic character of all deduction has prevented him from finding any better method. If he were to put all the reasoning of his science into syllogistic form, and state all the five propositions of each syllogism,[1] he would at one stroke discover all the primitive propositions of his deductive system and prove their independence—by a process that is deductive, as befits the nature of his science, and that does not need the extreme mental agility required by the other method.

The third requirement for a set of primitive propositions is that they should be "categorical" or "sufficient" or "complete." By this term it is meant that from them there should be deducible what is "essentially only one" system, or that any two systems of applied mathematics "that satisfy the 'set of primitive propositions' are capable of a one-to-one correspondence."[2] Just how much this requirement means as stated is difficult to say; for oneness is always relative, and the test proposed for categoricalness, namely, the isomorphism of the elements of two systems, is likewise relative, for what constitutes the "form" of a system is relative. Any system which is deducible from a set of primitive propositions, no matter how complex it is, may be said to be "one" with respect to that set of primitive propositions. Any two algebras or geometries deducible (with the aid of supplementary conventions, which are always necessary to "interpret" a theorem) from the same set of primitive propositions may be said to be isomorphic with respect to that set of primitive propositions, even if one is Archimedian and the other non-Archimedian. Euclidean and non-Euclidean geometry may even be said to be isomorphic![1] The requirement of "categoricalness" thus needs reconsideration.[2]

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[1] Cf. chap. vi, § 17.

[2] Veblen, "A System of Axioms for Geometry," *Transactions of the American Mathematical Society* (1904), V, 346; cf. *Monographs on Modern Mathematics*, pp. 191 f.; J. W. Young, *Lectures on Fundamental Concepts of Algebra and Geometry*, p. 49.

There are instead two things which are advantageous for the mathematician to know: that he has stated *all* the primitive propositions upon which his system rests (which is what the mathematician is really seeking in his requirement of "categoricalness" or "completeness"), and that the system is sufficiently abstract to be a branch of *pure* mathematics, i.e., a doctrinal function.[3] The first of these is the more important of the two and ranks with self-consistency as a fundamental requirement for a statement of primitive propositions to any deductive system. The only way in which to establish the completeness of a set of primitive propositions is to state the whole system, including the properties of the geometrical figures required for the demonstration of propositions,[1] in syllogistic form, and determine what are all the propositions in each syllogism. In this way all the presuppositions of any deductive system can be found and logical rigor attained. Such a logical analysis will show that no existing set of primitive propositions is complete —mathematicians have always (often explicitly) omitted the more general canons of deduction used to combine premises,[2] although there is no more guaranty that they will remain unchanged than that the parallel axiom will be unchallenged. It should be recognized that the assumptions upon which any deductive system rests include the canons of deduction used. Such an analysis of Euclid's geometry would show the logical difference between his postulates and axioms—the axioms may be used as canons of inference, while the postulates are material premises of inference.

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[1] There can be established a one-to-one correspondence between the elements of Euclidean and non-Euclidean geometry; each theorem in either geometry may be paralleled by a corresponding theorem in the other geometry, although the theorems would not be the same—such a correspondence may well be called isomorphism!

[2] If by "categorical" is meant complete, i.e., a collection of *all* the primitive propositions necessary to the deduction of a pure mathematical system, the term is misapplied, since no existing system of primitive propositions includes the general canons of deduction. Veblen, who first used the term "categorical," seems to mean by it a set of postulates which are sufficient to characterize what is considered as Euclidean geometry (*Transactions of the American Mathematical Society*, V, 346). Such a meaning has, of course, no such logical significance as the term "categorical" implies.

[3] Called by Mr. Keyser "pregnance"; cf. his *Mathematical Philosophy*, p. 115.

The other characteristic, that a system is really a branch of pure mathematics and not merely a branch of applied mathematics, may be shown by exhibiting two or more different systems which "satisfy" (i.e., are deducible, with the aid of a few supplementary postulates, from) the primitive propositions of the given system. This procedure is now usually carried out in connection with the statement of a set of primitive propositions.

We then sum up the logical requirements of a set of primitive propositions for a pure mathematical system as self-consistency, independence, completeness, and abstractness.[3]

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[1] Cf. chap. vi, § 26.

[2] This is true of Hilbert's postulate system and all others known to me. Even *Principia Mathematica* is deficient in the statement of its primitive propositions; cf. Appendix to chap. vi.

[3] Keyser has added the rather indefinite requirement of compedence; cf. his *Mathematical Philosophy*, p. 115. A much more important but non-logical requirement is usefulness, to which, indeed, almost all mathematical systems conform, but which is not necessary to such a system.

In accordance with this understanding of the nature of pure mathematics, as consisting in systems of deductions from sets of primitive propositions, the various branches of mathematics can be defined in terms of the sets of primitive propositions requisite for each of them. This sort of differentiation is indicated in the names for some of the branches of mathematics; e.g., non-Euclidean geometry is that geometry which is deducible from the primitive postulates of Euclid, except the parallel postulate. Then from the viewpoint of pure mathematics, ordinary real algebra and ordinary metric Euclidean geometry coincide, in that either includes the other.[1] The order of development may be different, but they differ in no other respect. Only when they are made concrete and the indefinables are given content does the difference between the two appear.

§ 4. In contrast to pure mathematics, which defines its fundamental terms only in part, applied mathematics defines them at least concretely enough to enable the primitive propositions of applied mathematics to be taken as a hypothesis and their consequences tested in experience.[2] The fundamental reason why pure mathematics does not define its terms, other than to state the way in which they combine in deduction,[3] is that a definition which does any more than state the logical characteristics of a term is entirely unnecessary for the purpose of deductively developing a system. An abstract definition of the sort given to an "indefinable" in pure mathematics[1] is quite insufficient for the practical use or concrete testing of mathematical propositions; consequently, applied mathematics can alone be said to be true or false. Applied mathematics then consists of the group of pure mathematical systems when the indefinables in their postulates are made concrete (i.e., made sufficiently definite to be identified unambiguously in experience), and which can consequently be tested as to truth or falsity. Ordinary Euclidean metric geometry or the algebra of real numbers are branches of applied mathematics.

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[1] Cf. Young, *Fundamental Concepts of Algebra and Geometry*, pp. 180 ff.

[2] From the mathematical point of view, such a definition is a "description," since it does not play any part in the deduction, but merely in the verification. Such a definition may be by a causal definition, if a conceptual nominal or essential definition is impossible.

[3] When an indefinable is used in pure mathematics, in order to maintain the abstract generality of the reasoning in form as well as in content, it would be well not to use any words, such as "congruent," as indefinables, since a word seems to be concrete and definable; instead, only symbols should be used, making a convention as to what abstract characteristics are to be understood by the symbol. In this way the distinction between pure mathematics and applied mathematics would be made clearer.

§ 5. This view that such a branch of applied mathematics as ordinary geometry is a rational inductive science[2] is sure to meet with a serious objection when we consider that the objects of geometry, such as points, lines, planes, etc., cannot be discovered in experience. According to modern molecular physics, atoms and electrons are always in motion and no such thing as a straight line can possibly exist.

It must be remembered, however, that no science deals solely with entities which are identifiable in experience. Physics, which is certainly an inductive science, deals with fictions galore—perfectly elastic and perfectly incompressible bodies, perfect fluids, ideally rigid bodies, weightless, unbendable, unbreakable, and unstretchable levers—yet physics is hardly a pure creation of the mind! The human mind is often unable to deal conveniently with the complexities of the real world, hence it creates such fictions as will enable it to state the properties of the real world in an approximate and simple manner, and then, by combining with such a fiction (as that of a weightless and unbreakable lever) other conceptions (such as the degree of elasticity of an iron bar, etc.), it arrives at a statement that is verifiable by comparison with what is found in experience. In some cases the real world is so complex that we have not yet arrived at an exact rational statement of the complex behavior of objects, but merely at an approximation thereof. Our approximations, however, are so close and verified in such different fields that we recognize their truth and see that by a process of successive approximation it will be possible to reach an exact statement of the observed behavior of the object. These approximations are, accordingly, limited universals—limited to the degree that they are merely approximations.

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[1] A mathematical "indefinable" is not in reality completely undefined; for, if so, any meaning whatsoever could be given to it when giving a concrete meaning to a system; it is just *relatively* undefined—the "indefinable" is given meaning only with reference to its logical characteristics, especially what sort of an entity it is (a dyadic, triadic relation, etc.) and how it acts in deduction. For example, the term "congruent" as used in a pure mathematical system could not be made concrete by giving it the meaning "fishy"! "Congruent" is a dyadic relation, and can be made concrete only by another dyadic relation. One of the requirements for strict logical rigor in a pure mathematical system is that its "indefinables" should be defined so far as is necessary for the development of the system (i.e., to indicate how they behave in deduction) and for the definition of what sort of entities can be used to make them concrete when the pure mathematical system is made into an applied system.

[2] Bôcher, however, states that this view "has been held ever since Gauss's time by almost all the leading mathematicians (except Poincaré) who have been conversant with non-Euclidean geometry" (*Bulletin of the American Mathematical Society* [1904], XI, 124).

Geometry must be considered as an inductive science that deals wholly with ideal objects of the sort with which physics sometimes deals. While the point, line, plane, etc., do not exist in experience, yet they are plainly the *limits* of the objects which are identifiable in experience; as Descartes said, "Geometrical truths are in a way asymptotes to physical truths, that is to say, the latter approach indefinitely near without ever reaching them." The ancients who first formulated the science of geometry drew their figures on the sand—their figures must have been very rough and inexact—yet they saw their theorems in their figures, and used intuition very widely in proving their theorems. A chalk line on a blackboard is an approximation to a geometrical line, and serves very acceptably for the demonstration of a geometrical theorem, because the spectators recognize that what is referred to is what the chalk line would become at the limit when its breadth is indefinitely decreased. Geometry is consequently a verifiable science, not because its objects are found exactly in experience, but because in the case of geometry, as in the case of other sciences, objects can be found whose limits are the objects of geometry; the final verification of geometry does not lie in the fact that any real objects exactly obey its laws, but that, when geometrical laws are *compounded with* other laws of physical objects, the resultant is exactly the behavior of observed objects. The truth of each of the component laws of such a resultant can be determined independently, since these laws enter into different combinations and each law can be verified separately by a wide range of deductions. Geometry is, accordingly, a limited universal—limited by the requirement that its laws must be compounded with other laws before they can be exactly verified.

Many of the laws of physics are of this ideal sort. When Galileo propounded his law of falling bodies, he recognized that the implication that all bodies fall with equal velocity was not fulfilled; but he knew that, as the weight of a body increased and the cross-sectional area decreased, this law is better and better obeyed; consequently he was right in reasoning that, if the resistance of the air were eliminated, this law would be exactly obeyed. Thus the law of falling bodies was made into a fiction which Galileo could not directly verify, but which could be tested by composition with a law of air-resistance. Later experimenters were able to remove the air from a tube and verify Galileo's conclusion directly. In the case of the law of inertia, Galileo found that as his groove and ball were made smoother, the ball would roll farther; hence he reasoned that at the limit the ball would continue in the same direction and with the same velocity indefinitely—his successors have never been able to verify Galileo's reasoning directly, and the law of inertia remains a statement for which, because of the accidents of experimental method, physicists have not been able to provide a direct proof—but it remains just as true as the law of falling bodies, because the law of inertia and the other laws with which it is combined in order to obtain an experimentally verifiable deduction, can be individually combined with different other laws and thus be verified, each separately in a wide range of phenomena. So with the laws of applied geometry—they can only be verified in composition with other laws, yet verification is possible for them, and geometry may be pronounced an inductive science.

The statement that applied geometry is an inductive science must not be interpreted to mean that every proposition in Euclid is exactly verifiable. It is a peculiarity of deduction, and one which is extremely valuable, that it is possible to make the deduction from any hypothesis more exact than any possible exactness of verification. Otherwise we should frequently be at a loss to know, in a discrepancy, whether our deduction or our verifying measurement is at fault. The hypotheses of geometry, it must be remembered, apply exactly to nothing; they are approximations or statistical results which hold true of physical objects only in the gross. Consequently, we must not expect the exact propositions of such a deductive science as geometry to hold true to the last decimal place any more than we should expect the law of falling bodies to hold true to the thousandth part of a second or of a millimeter without taking into consideration other factors, such as the resistance of the air, etc. The inference that, because the value of π has been deduced to 707 places of decimals, therefore the circumference of a physical circle 1 meter in diameter is exactly 3.14159265358979323846 . . . . meters is absurd, for we have failed to take into consideration the diameter of the atom and the electron! Geometry is a limited universal whose limits we are just coming to recognize. When the absurdity of such exactness is recognized, even the opponents of the truth of geometry must confess that in so far as measurement or other verification of geometrical propositions is possible, the consequences of the hypotheses of geometry are verified. Verification can be carried into so many diverse fields without exception that we become convinced of the truth of geometry as a statement of the gross behavior of material bodies, even to a somewhat greater extent than the present refinements of measurement.

§ 6. The non-Euclidean geometries, especially those of Lobachevsky and Riemann, raise a different question, because these geometries seem to be alternative hypotheses whose deduced consequences cannot be experientially distinguished from those of Euclidean geometry. Consequently, it is said that we are driven to use the principle of simplicity and choose Euclidean geometry as being the simplest.

For practical purposes, it is undoubtedly permissible to choose the simplest hypothesis, just as a great many other departures from logical rigor are often justifiable for purely practical purposes, such as the assumption that a divisor is not equal to zero or that a series is convergent. But the scientist dare not take refuge in the principle of simplicity, for it has too many times proved itself a mirage and hindrance to progress in a world which seems more and more complex, the more we know of it. What then shall we say about the truth of Euclidean geometry?

It must be remembered that the metrical properties of non-Euclidean geometry depend upon a parameter k; that when k becomes infinite, the non-Euclidean geometries reduce to Euclidean geometry; and that in proportion as *k* becomes larger, the properties of the non-Euclidean geometries approach the properties of Euclidean geometry. That is the reason why it is impossible to distinguish between these geometries in experience—this parameter *k*, which depends on the size of the universe, must be taken so large that the consequences of these different geometries become practically indistinguishable.[1] If the size of the universe could be proved to be infinite in all dimensions (and its properties uniform throughout), the Lobachevskian and Riemannian geometries could be eliminated; we do, moreover, know that the universe is extremely large.[2]

[1] A universe such as that suggested by Poincaré, in which the index of refraction varies with the absolute temperature, and which would consequently exhibit the characteristics of a Lobachevskian geometry without being infinite in size, could be recognized by making a *wide* deduction from the hypothesis that the absolute temperature varies, e.g., comparison of melting-points at different places would show that (while the melting-point of the substance remains the same absolutely) the thermometer registers different melting-points at different places. If the melting-point did not serve, some other physical characteristic could be used. In this fashion the variation of the absolute temperature could be determined and a correction made for it in calculation. Thus the Lobachevskian characteristic of the universe could be established.

[2] Einsteinian relativity, as far as it has been verified (by the aberration of light passing near the sun or the displacement of lines in the spectrum), does not presuppose a *general* curvature of space, but a *local* curvature in the neighborhood of matter. It does not then imply (in so far as it is verified) a uniformly curved space, such as the Lobachevskian or Riemannian geometries imply, but regions of non-Euclidean curvature, which are, moreover, compatible with the Euclidean character of space remote from matter. Hence the theory of relativity does not aid us in deciding between these geometries. It is furthermore possible that the phenomena Einstein supposes due to this local curvature of space may be found to be due to a different reason—to the effect of matter rather than the nature of space. An adequate search for possible alternative hypotheses has not been made in connection with the theory of relativity, because of the highly abstruse mathematical nature of such hypotheses. Einstein's theory of the cylindrical universe is a later and as yet unverified speculation, and so does not offer any means of testing the Euclideanism or non-Euclideanism of the universe.

The ultimate reason that these curved non-Euclidean geometries cannot be eliminated as possibilities is that it has been impossible to give to the concept of

Euclidean geometry is the limit to which the other geometries approach as the parameter k increases. Geometry, moreover, does not deal with the physical world directly—it is only true of physical lines and points at the limit—so it would seem to be only consistent with the nature of geometry to take the limiting geometry as the correct one. Otherwise these three geometries must remain as alternative hypotheses, between which we cannot, in the present state of knowledge, decide which is true.

§ 7. The ultimately hypothetical and inductive nature of mathematics is illustrated better in the algebras than in geome-

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the straight line a scientific definition that is more than approximate, i.e.,we have not as yet been able to specify a definition of a straight line in *physical* terms that will enable us to distinguish in *all* cases, in ultramicroscopic as well as astronomical phenomena, whether a given line is straight or not. The discovery of such a physical definition for the straight line will solve the difficulty responsible for these geometries, by enabling us to tell which one applies in any given case. In default of such a definition we are reduced to the method of trying these geometries and seeing which one fits a wide variety of observed phenomena best.

As a matter of mathematical calculation, any phenomenon that is said to require a non-Euclidean geometry for its description may be equally well described in a Euclidean space of a higher number of dimensions. (Cf. A. d'Abro, *The Evolution of Scientific Thought*, p. 61.) A four-dimensional non-Euclidean phenomenon could be completely fitted into a ten-dimensional Euclidean space. Consequently, the difference between non-Euclidean and Euclidean geometry, when used in physics or astronomy, is merely a difference of convenience in calculation, which may turn out to be only a slight difference, if any, since the general equation for the differential of an infinitesimal distance in a variable non-Euclidean space of four dimensions requires ten components, while the differential of the same distance in a Euclidean space of ten dimensions requires the same number of components. If, then, it makes no physical difference whether a phenomenon is stated in terms of Euclidean or non-Euclidean geometry, our criterion that the limiting (Euclidean or homaloidal) geometry should be used to deal with phenomena concerning spatial limits (points, lines, etc.) is pertinent and should be applied.

These considerations indicate that the logical foundations of Einsteinian relativity (since that theory is based upon the use of non-Euclidean geometry) are very likely weak, and that either additional facts need to be presented or the theory may be interpreted in quite different terms. In its present form, the theory of relativity does not aid us in dealing with the non-Euclidean geometries.

try. It is noteworthy that, of the many algebras which are possible, only a very few have been selected, because they are practically useful and true of the world with which we deal. The associative and commutative laws, in particular, are true of the first counters, pebbles. We should remember that in the development of arithmetic the analysis which resulted in the statement of these two laws came very late in the history of algebra; yet, long before they were stated, it was generally accepted that $2+3=3+2$. The behavior of pebbles is unmistakable, and so these laws were assumed centuries before they were stated—their *experimental* truth was so evident that no need was felt for a rational justification of them. A law is not usually stated until a contradiction appears; so it was only in modern times, when systems which do not obey the associative law were invented, that these laws came to be vividly important. The arithmetical quality of denumerability is possessed by most physical objects, just as color and figure is, but it is not invariably found. Fluids, for instance, do not in themselves have the quality of denumerability, just as they do not have figure. But denumerability is of such great practical importance that we have artificially introduced it into fluids, and speak of so many gallons of water, etc., just as we ascribe to a fluid the figure of its container. Then we can say of the algebras that they are true wherever the presuppositions which they assume are correct. Moreover, just as the deduced consequences of geometry must be combined with other physical considerations before they can be tested in experience, so denumerability (i.e., the possibility of making a one-to-one correspondence between the number series and objects) must be combined with other characteristics of a set of objects before it can be tested and found true. The various algebras, of real, irrational, imaginary, or other numbers, like geometry, may then be said to be true when applied to the right sort of subject matter, because their deduced consequences are found in experience.

§ 8. If applied mathematics consists of those deductive systems that are verifiable in experience and are large and comprehensive, it is evident that all sciences, in so far as they become developed and unified, will become mathematical in the broader sense of the word, that is, deductive. That does not mean that they will become quantitative, for modern mathematics has abstracted from quantity. It merely means that, since deduction is an essential part of rational induction, and since the development of a science consists in the formulation and proof of larger and more inclusive hypotheses, deduction will become more and more comprehensive as hypotheses become more inclusive, so that all sciences (unless the world is fundamentally pluralistic) will become deductive and fundamentally one. Mathematical physics has strikingly shown this sort of development. Since physics deals with phenomena which can be analyzed into simple hypotheses better than any other discipline (except perhaps astronomy), it has progressed further than most sciences; consequently, it has become mathematical. Other sciences also tend to show the characteristic of making a maximum of deductions from a minimum of assumptions. This tendency does not make these sciences part of mathematics—for the fundamental hypotheses of each of the sciences differ from those of mathematics in being more concrete, whereas mathematics keeps its most concrete assumptions somewhat abstract. A deductive science may be defined in terms of its fundamental hypotheses, and so distinguished from other deductive sciences. The sciences will become mathematical, but not mathematics.

§ 9. In conclusion to our discussion of the logical nature of mathematics, it may be enlightening to give a résumé of the logical history of that discipline. As we should expect from a recognition of the truth of applied mathematics, it began as a purely empirical science (dealing with empirical generalizations only). The Egyptian rope-stretchers, in order to determine a right angle, used a simple triangle which obeyed the Pythagorean proposition. The pyramid builders constructed empirical tables of natural cosines. Simple algebraic problems were solved by empirical means. Not until the Greeks took up the subject did anyone think of making a few self-evident assumptions and deducing the known and other geometrical properties from those assumptions. That discovery changed the logical nature of the science completely; it now became a rational inductive science, deducing its results from hypotheses which were thought to be self-evident.

Once this procedure was started, two results were inevitable: in the first place, it was found possible to extend the boundaries of knowledge so rapidly by mere deduction that great emphasis was put upon deduction, and even applied mathematics came to be thought of as a purely deductive science, to the great mystification of philosophers. In the second place, the assumptions of the science were analyzed and corrected. The Pythagorean point, which had magnitude, was replaced by the "mathematical" point, and the continuous line was introduced, in order to avoid consequences that were contradicted by experience, which Zeno showed to be deducible from the formerly accepted assumptions—the discovery of inductive exceptions forced a restatement of the hypothesis. Finally, a single set of postulates and axioms was produced for geometry, from which geometers supposed the whole of the science was deduced—an ideal which is just being achieved.

The history of the remainder of the ancient period and of the medieval period of mathematics consists in a further development of deductions from the same assumptions (many of which assumptions were, however, only tacit). The modern period is also characterized by further developments of this sort, especially by the development of the extraordinarily powerful infinitesimal calculus, and the further refinement and correction of the fundamental hypotheses of mathematics by the exact definition of such terms as continuity, etc.

Modern mathematicians have done more than that—they found that the same statements could be used (with a slightly different meaning of their terms) to state the characteristics of different fields of experience, such as those of geometry and pairs of numbers, so that there finally developed pure mathematics which purposely leaves its terms mostly undefined, for the purpose of giving them larger application, and develops only the abstract formal side of mathematics. On the other hand, some mathematicians tried the experiment of altering some of the propositions in the fundamental hypotheses of mathematics, and, because the conception of pure mathematics had relieved them of the necessity of seeking for experiential verification of the results of their assumptions, they proceeded to develop various other types of mathematical systems, such as the non-Euclidean geometries, quaternions, hypernumbers, hyperspaces, etc. Once developed, uses were found for many of these new systems, and so further elaboration ensued. In this way there came about the complex mathematics of the present day, partly pure and partly applied, partly resting upon mere assumptions, and partly verified through its deductions; partly based upon experience, and partly making assumptions drawn by analogy from experience but unverifiable in any experience.

§ 10. Nowhere has science been led further astray by the faulty logical theory that has been so prevalent than in the theory of probability. In this field, those who have been best acquainted with logical theory have often been misled, while some who have daringly avoided logical problems have reached a sound theory.

The reason for this anomaly is not hard to find. When Locke and Hume rejected sense experience as a basis for knowledge, they nevertheless found men depending upon sense experience in practical life; since experience does not give certainty, Locke followed the ancient Skeptics in concluding that it gives probability, and Hume followed him in making this uncritical assumption. To Hume and his followers, the only possible form of induction was purely empirical; rational induction was unknown. Probability, which alone was supposed to deal with sense experience—the basis for the practical guidance of life—cannot then be founded upon any induced certain truths, for induction depends upon it. Hence probability cannot be objective—it cannot make any statement about the nature of things; it can only be subjective, a statement about human feelings, just as causality became a subjective feeling to Hume. The Humean analysis of induction thus logically prevents probability from asserting any objective ground, and confines it to a statement of purely subjective expectations.

Yet the theory of probability is certainly not derived from psychological expectations of events. It is rare that a gamester becomes so philosophical that his personal expectations coincide with the results of mathematical probability. What gambler expects to throw double sixes only one thirty-sixth of the time if he really expects to win? If a gamester succeeded in making his feelings thus "reasonable," he would give up his avocation! Human prejudices and emotions of self-confidence or despair almost always cause subjective expectation to be either greater or less than that justified by mathematical probability.

To meet this objection, the upholders of subjective probability have asserted that "the theory does not measure what the belief is, but what it ought to be,"[1] i.e., that it "is concerned with the degree of belief which it is *rational* to entertain in given conditions."[2] Then the question arises: If probability does not deal with actual human expectations, except those of the man who has severely disciplined his emotions to correspond to the theory, so that it deals only with ideal "rational" expectations, how are we to determine what is the "rational" degree of subjective expectation? That degree can only be an ideal normative expectation, which, it is admitted, is rarely found in human experience! We then need to ask what are the bases upon which such a standard for human expectations is founded. The supporters of the subjective theory have divorced that standard from experience; for them, it cannot be founded on any experience, because the theory is not amenable to verification or disproof from experience. Is it founded on a priori certainties? But Hume, the father of subjective probability, destroyed all such a priori truths. Then the foundation of this normative standard of ideal "rational" expectation is merely a set of arbitrary dogmatic assumptions, many of which, such as the principle of non-sufficient reason, are themselves of doubtful value!

The supporters of the subjective theory are not, however, content with such a consequence of their theory. Instead, they secure a foundation for a statement of probability by making observations of the known features of the case—the number of faces of a die, etc. In other words, subjective expectation, to be "rational," must become objective! True, there are some things unknown; it is not known which face of the die will be uppermost, but some things must be known with *certainty* before even an expectation of one-sixth for any face of the die becomes "rational"—that the die is going to be known, that it is correctly made and fairly thrown, etc. But, for a Humean logic, how is such *knowledge* possible?

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[1] Jevons, *Principles of Science*, p. 199.

[2] J. M. Keynes, *A Treatise on Probability*, p. 4.

In the absence of *any* certain knowledge, how can any finite probability be "rational"? Only a statement of absolute ignorance would seem to be in place. Without any certainty at all, any statement of a probability is itself only probable, and that statement itself again only probable, until the mathematical total probability (if we assume the mathematical laws for calculating probability) takes the form of an infinite product whose terms are all proper fractions—such a total probability approaches zero as a limit! Unless there is some foundation of certainty underlying every statement of probability, all probabilities become merely probably probable, and, when the total probability is calculated, it is found to be zero or ignorance![1]

The very fact that the theory of probability is a branch of mathematics and is therefore deductive shows that, like all other branches of mathematics, it rests upon a set of primitive propositions which are asserted to be certain. Without that certainty, the whole theory is groundless. In order to convince ourselves that probability is not merely subjective, we have only to ask ourselves the question: Suppose men had found that one face of a fairly made and fairly thrown die comes uppermost half of the time instead of only one-sixth of the time, would it continue to be "rational" to expect a probability of only one-sixth for that face? We then see that the theory of probability is not merely a normative standard of subjective expectation, but one which is based on factual and objective certainties. Probability, just as any other branch of applied mathematics, is a case of rational induction. Our theory of certainty as obtained by rational induction moreover obviates the necessity, derived from the Humean analysis of induction, of founding induction upon the theory of probability or of forbidding the assertion of any certain knowledge as the basis of a statement of probability.

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[1] Cf. chap. iii, § 28.

§ 11. How did such an objective conception of probability arise, and what is probability, if it is primarily an objective statement verifiable in experience, and hence only secondarily a matter of "rational" expectation? It is a fact of experience that some events seem to be irregular if taken separately, but become regular if taken in groups. This fact seems to have first been noticed in connection with games of chance. It is impossible to predict accurately which side of a die will come uppermost, yet, given a sufficiently large number of throws, each side will appear an equal number of times. Not only the artificially simplified conditions of games of chance, but such individual events as deaths, marriages, divorces, suicides, the number of letters annually sent to the Dead Letter Office, peak loads in telephone traffic, the sale of small standardized machine parts, become regular when a large number of cases are taken. In this way it is possible to make a true statement about a group of events when it is impossible to say anything about individual events. While it is impossible to predict that an individual may be either alive or dead a year hence, it is possible to predict that, out of a thousand inhabitants of a large city, a definite number will be dead within a year. The fundamental reason for this statistical behavior of individually unpredictable events is very likely that their causation is extremely complex, and that, by taking a large number of events, individual factors cancel each other, so that only the mutually common factors appear in the resultant.[1]

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[1] Such studies as the relation of broken homes to child delinquency, of the effect of a serum upon cases of a disease, when checked against a control group, show that, given a sufficiently large group of cases, such factors as inherited tendencies to crime or to a useful life, or natural resistance to disease or susceptibility to disease, cancel each other, and only the effects of the factors with which the groups are not heterogeneous, such as broken homes or the serum, are found in

We might possibly be close to the truth in saying that chance behavior occurs when there are a number of influences affecting an event, each of which varies slightly in degree *independently* of the others, so that sometimes these influences reinforce each other, sometimes they cancel each other, and sometimes they partly reinforce and partly cancel each other. For example, the United States artillery in France estimated that it takes two hundred shells from one battery of four guns to destroy an enemy battery of four guns, *when the guns are pointed accurately*. It is likely that ample allowance was made in that calculation so as to be sure of a hit, nevertheless, the implication, that it takes fifty shells to be sure of hitting one spot, is astounding until we realize that we are herein dealing with a chance phenomenon. Gusts of wind vary in all directions; even a steady wind may vary slightly in strength; the amount of powder may vary slightly, so may the weight of the projectile; the distance the shell is thrust into the breech may vary slightly, giving more or less space for the powder to explode—the effect of each of these and other factors is magnified by the great distances to which modern shells are thrown. These factors vary independently of each other and therefore produce a chance distribution in the resultant hits, grouping them around a given point, but making it possible for individual shells to hit different points for the same placing of a gun. In like fashion, other "chance" occurrences may be due to a similar sort of complex causation to which independently varying influences contribute.

The probability of an event is then the proportionate number of times this event or one like it will occur in a sufficiently long series of events of a particular kind that is specified by a definite set of conditions, when such a series of events is charac-

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the net result. This principle of the mutual canceling of heterogeneous, individual factors in a large number of cases is the fundamental principle in the use of statistics for determining causal connections.

terized by statistical regularity. There are three important features in this definition of probability: (1) Probability may be stated as a numerical ratio of the number of times the event under consideration or one like it occurs to the total number of events in the group of events. (2) This group of events must be chosen sufficiently large so that it will show statistical grouping, i.e., a (not necessarily ordered) regularity of occurrence of the various sorts of events within the group of events, and this statistical regularity must occur. (3) The group must be defined by certain conditions; if these conditions change, the group no longer exists and the judgment of probability no longer applies.

Probability thus, as Laplace said, "has reference partly to our ignorance, partly to our knowledge."[1] We are ignorant of all the causal factors determining our probable event, but we know enough to place that event in a series of events which has a definite unordered regularity of occurrence. Unless we have that knowledge, even a statement of the probability of the event is not "rational," for a statement can only be rational when it is justified by *some* knowledge. Ignorance justifies nothing except a confession of ignorance.

It is, of course, true that we make many estimates of probability which are not rational, in that they are not based upon sufficient knowledge. Even these judgments of probability, which we recognize as only approximate, can be interpreted as meaning a statistical regularity. When I say that N.N.'s chance of becoming heavy-weight champion of the world is 3 to 5, my meaning may be interpreted to be that, of a sufficiently large number of men like N.N., in situations and with opponents such as his, three-fifths of them become champions.[2] Such a statement can rarely be verified; but analogous statements are verifiable, such as, "The probability of my dinner's being ready when I get home is 5 to 6," which means that, given the present set of conditions, cook, etc., my dinner is ready, on the average, five times out of six. Or, if no numerical ratio is given, the judgment may be phrased, "My dinner will probably be ready when I reach home." Analogous to that are such statements as "A.B. is probably crazy," meaning that, of people who act as he does, most are crazy; or "C.D. will probably be elected president," etc. In this way our judgments of probability arise out of the experience of statistical regularities, and have no direct reference to any particular event, but merely attribute that particular event to a class whose regularity is exactly or approximately known. Probability is objective and statistical, and only thus can become "rational."

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[1] *Essai Philosophique sur les Probabilités*, fifth Paris edition, p. 7.

[2] This is not, of course, the bookmaker's meaning of probability; he is concerned with the number of bets and the amounts bet on each eventuality, rather than with the events themselves.

§ 12. There is, however, another meaning of the word "probability" in common use, which must be distinguished. Very frequently, probability is said to be secured in an inconclusive argument, in which the conclusion is not proved to be certain, but merely shown to be possible (i.e., it is shown that the impossibility of the conclusion cannot be established). Here there is no reference to any statistical frequency at all. We cannot, of course, forbid the popular use of terms in ambiguous meanings, but when there are two different words and two different meanings, it is well that, in scientific usage, each meaning be kept distinct. Consequently, we urge that when there is a direct or remote reference to statistical frequency, the term "probability" be used, but when there is no such reference, the statement be said to be "possible" rather than "probable."

A judgment of probability does not concern itself directly with any individual, but merely with that individual as a member of a class; whereas a judgment of possibility refers to the individual (judgment or object) directly. Arguments whose truth is not quite but nearly established may be called "very possibly true" rather than "probably true." It is too bad that two different meanings should be so confused as the meanings of possibility and probability have been. The reason is that both lead to subjective expectations which vary in degree, and, as a result of Hume's argument that truth is merely degree of belief, the two have been confused.

When we come to apply the theory of probability to a merely "possible" statement, we lay ourselves open to many absurdities. What is the "probability" that the conclusion of a syllogism with an undistributed middle is true? Shall we say that because "all men are living creatures" and "all animals are living creatures" there is a "probability" that "all men are animals"? Then what probability shall we assign to the conclusion of the two premises, "All men are animals" and "All dogs are animals"? Probability cannot be a substitute for logical rigor in argument, nor is a conclusion probable that "partially follows from . . . . a premiss,"[1] unless we are ready to accept a great deal of nonsense. If we are referring to the fact that a certain proportion of conclusions from syllogisms with undistributed middle terms are themselves quite true, we could assign a probability, but that probability would not belong to any one syllogism, only to the group of that sort of syllogisms. We might say that of all Aristotelian syllogisms with an undistributed middle, 25 per cent give true conclusions, therefore such a conclusion has the probability 1 to 4 of being true—but such a statement does not say anything directly about that conclusion any more than the statement that I have 8 chances out of 10 of living another year tells me whether I am going to die or live —I may do either quite consistently with that statement. We see by these absurdities that the theory of probability is not a means of deriving knowledge from ignorance, although a popular use of the word seems to indicate that. A statement of probability can only be made on the basis of a certain amount of *knowledge;*[1] out of ignorance only ignorance can be evolved. We had thus better distinguish these two sorts of judgments by saying, for example, that the nebular hypothesis is *possible*, while my being run over by an automobile is *probable*.

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[1] Keynes, *op. cit.*, p. 52.

§ 13. Some scientific probabilities are still entirely or largely empirical and statistical statements. Such are mortality, birth, and marriage statistics, life insurance tables, etc. Such probabilities plainly say nothing about individuals, but only about groups of persons, and can hardly be interpreted subjectively. In other cases, especially in those in which the situation is comparatively simple, such as games of chance, it is possible to make a hypothesis and deduce from that hypothesis marvelously fertile results. For instance, it is usually asserted that when the forces tending to produce any one of a number of alternative events (such as those operating in a game of chance) are equal or have equal fields of variation, these events are equally likely, and the probability of such an event may be expressed by a ratio, the numerator of which is the number of that sort of events that are possible, and the denominator the total number of possible events in the group. This hypothesis is justified by its verified consequences—under these conditions we find the statistical grouping that is characteristic of the experiences from which we derived the conception of probability.

From this hypothesis are deducible the addition and multiplication theorems of probability,[2] and ultimately Bernouilli's theorem and the rest of mathematical probability. The converse hypothesis, that wherever we find statistical grouping, there the forces operating are equal or have equal fields of variation, is likely not true, for it is quite possible that statistical grouping might be caused in several different ways. Nevertheless the phenomena observed are so similar in their fundamental nature to those observed in connection with games of chance in which the forces are experimentally made equal, that it brings about no great departure from the known facts of experience to make the same assumptions wherever statistical grouping is found. Indeed, it is likely that when we have no knowledge of an actual attempt to equalize the forces tending to produce different events we judge that different events are "equally likely" by the statistical grouping of those events, and thereupon apply the principles of mathematical probability. We need not know what are the causes which produce these "equally likely" events; if we can determine the conditions under which statistical grouping occurs, we can apply the theory of probability to the events which happen under those conditions.

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[1] Keynes, *op. cit.*, p. 4, admits this fact: "Given the body of direct knowledge which constitutes our ultimate premises, this theory tells us what further rational beliefs, certain or probable, can be derived by valid argument from our direct knowledge."

[2] Cf. J. L. Coolidge, *Introduction to Mathematical Probability*, chap. ii.

In this way a thoroughly objective and statistical view of the nature of probability can be made self-consistent and a meaning can be given to the theory of probability, which can be verified or disproved in experience.

§ 14. The best attack upon such an account of the theory of probability is to be found in Mr. Keynes's *Treatise on Probability*.[1] His fundamental criticisms are three: (1) since an objective determination of probability consists in the assignment of an event to a class whose truth-value is known, we have the difficulty of making this reference. (2) This theory of probability leaves the fundamental theorems of probability, especially the addition and multiplication theorems, inexplicable. (3) It makes probability depend upon induction, whereas induction really depends upon the theory of probability.

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[1] Cf. chap. viii, especially sec. 11 and following sections of his book.

This third criticism is undoubtedly the most important one for Mr. Keynes, as is natural in the case of one who holds the Humean view that the only method of knowledge is by empirical induction. Rarely have the supporters of this view seen, as did Hume,[1] that any probability achieved by this method is itself only probable, and eventually becomes infinitely small. Hence the method of probability, when applied to Humean induction, produces the result that it is infinitely improbable that there should be any finite probability! But once we have seen that rational induction does not depend on probability, that it is not established by the method of probability, we can accept as correct the conclusion that probability depends on induction and not vice versa, and see in it a vindication of our theory.

As to the second objection, the theorems of probability, including the two mentioned, are deduced from the fundamental assumptions of the theory of probability by the use of the infinitesimal calculus.[2]

Just what Mr. Keynes means by the first objection, I cannot make clear to myself; there seems to be equal difficulty in assigning any particular object to membership in a class: an individual chair has its peculiarities so that it is not exactly like anything else, hence it would seem to be *sui generis;* yet we find no difficulty in assigning it to the class of chairs. It must be remembered that a class which has a statistical frequency is a *class*, not an individual, and that we assign other individuals to the class on the basis, not of their total characteristics, but of those which they have in common with the class, i.e., the relevant ones. Of course such an assignment is always made on the basis of the evidence at our disposal—it cannot be made otherwise; all inductions are made in that way. The ancients decided that the earth was flat, and we may admit that as far as their knowledge went, they were excusable for drawing such a conclusion; but that does not render their conclusion *true*, because that judgment pretends to concern itself, not with their evidence, but with the objective earth; we criticize them for not securing more evidence. So with judgments of probability: on the basis of a certain set of evidence, an event may be assigned to a class having a certain frequency, but, unless such an assignment can be as conclusive as a rational inductive conclusion should be, we cannot be sure that our assignment is correct. The method for determining what is the material basis for drawing a conclusion concerning the probability of an event is precisely the same as that for drawing any inductive conclusion, and should be equally guarded. Just because a statement is of probability, its logical grounds should not be less exact than other inductive conclusion. Of course, there are many cases in which we can reach only an approximation to the truth; they should be recognized as approximations, themselves having an unknown degree of probability or possibility, to be compounded with the resultant probability. A correct understanding of the theory of induction thus removes all the important objections to the acceptance of an objective theory of probability.

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[1] Hume, *Treatise,* Bk. I, Part IV, sec. 1, p. 182.

[2] Cf. Coolidge, *Probability,* chap. ii.

The other view, that probability is wholly subjective and has no reference to objective experience, leads to a host of self-contradictions (some of which have already been mentioned) and makes the theory of probability meaningless. It also leads to the assumption of the principle of non-sufficient reason. If probability is subjective only, i.e., relates to our knowledge, and concerns itself with "a logical relation between two sets of propositions in cases where it is not possible to argue demonstratively from one to the other,"[1] then the less we know about our evidence, the more equal the probabilities on each side would be, and, if we know nothing at all on either side, the probability would be 1 to 2! In reality we should be utterly ignorant, and a statement of probability would be in no way preferable to any other statement. The principle of non-sufficient reason attempts to answer the difficult question as to when different alternatives can be said to be equally likely; it asserts that when the *evidence* is "symmetrical with regard to the alternatives, and is applicable to each in the same manner," "then relatively to such knowledge the assertions of each of these alternatives have equal probability."[1] Even this statement gives us the absurd result that the probability of all things of which we are ignorant is one-half. Then if I am totally ignorant of mortality tables, etc., the probability that I shall die tomorrow is 1 to 2, but if I do know them, the probability is 3 to 100,000!

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[1] Keynes, *op. cit.*, p. 8.

It is quite correct to say that the subject matter of probability is propositions, not events, and it is correct to say that probability is always relative to certain evidence, and that, if the event does not happen as predicted, my judgment of probability *relative to that evidence* remains correct (i.e., it is a *valid* deduction, though it can hardly be *true*); but it is not correct to say that judgments of probability refer only to other judgments and not to events. All mathematical equations are, logically considered, judgments or propositions, statements of probability included. On the basis of a certain set of evidence, I may assign a particular event to a certain class with a statistical frequency, a/b; then experience may show me that this event does not happen a times out of b; yet I may have been quite correct in stating the frequency of the class of events I was considering; my mistake was not in giving that numerical frequency to that class of events, but in failing to gather more evidence and consequently assigning the particular event to the wrong class. Similarly I may judge that *as far as my evidence goes*, I am quite within my rights in hunting on a certain piece of land; yet that judgment, even if it is a correct deduction from the reasons I have for holding it, will not preserve me from arrest and fine if those reasons happen to be incorrect! I am punished, not for drawing an incorrect conclusion from my evidence, but for failing to inform myself sufficiently. While a judgment of probability with regard to certain evidence may correctly be a/b, yet that fraction may be quite incorrect as a statement of the probability of a particular event, because it is assigned to the wrong class. The important question is not whether the subject matter of probability (in the sense of that which *constitutes* calculations of probability) is judgments, but whether the subject matter of probability (in the sense of what it *refers to*) is merely human judgments (psychological expectations), or whether it refers to *events*. This ambiguity of the term "subject matter" has undoubtedly misled many. It is admitted that probability does not refer to any actual expectations, but only to ideal "rational" expectations; then either such "rational" expectations are purely arbitrary and logically unjustified, or they are based (mediately or immediately) on experience. The ordinary phrasing of a judgment of probability, that "the probability of X's living five years is 4 to 5," shows that the reference is always to some *event*, not merely to an expectation. If the reference were only to expectations, and if expectations have nothing to do with actual happenings, it would seem pragmatically rational to adjust the theory of probability to give only pleasant probabilities, instead of making it independent of human hopes and desires! The fact that judgments of probability are always relative to certain evidence does not necessarily make them subjective.

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[1] This is Keynes' revised definition, *op. cit.*, pp. 56, 42.

The adherent of the subjective theory of probability is nearly always a practical man as well as a theorist, and so, even while admitting that "the Laws of Probability . . . . cannot be really negatived by any possible experience"[1] and are amenable to no experience, he sometimes makes experimental tests of his non-empirical theory! Jevons[2] made a remarkably arduous test. Even Mr. Keynes obtains, from discrepancies between the predicted and observed results of probabilities, real "knowledge of the material construction of a die or of the psychology of a croupier"![3] An ideal subjective expectation that has no relation to any objective events would be a mathematical pastime that is worthless for science and for practical life—but the theory of probability has been put to too many practical uses to be what the subjectivist thinks it is. No wonder Mr. Keynes found that "it is difficult to find an intelligible account of the meaning of probability."[4]

§ 15. The objective theory of probability, which is here defended, is not, mathematically considered, a part of pure mathematics, but rather a part of applied mathematics. It is indeed important to know just where the theory of probability can be applied and where not; for all the special sciences that need to use the theory of probability, this is a very important problem. In the present state of the theory, it is unfortunately necessary to devote most of our energies to settling this problem. But, for the mathematician as such, the concrete applications of his theory are not his concern; he is merely interested in developing the implications of a given set of fundamental assumptions. So, once the fundamental assumptions of the theory of probability are settled, the mathematician can go on developing his theory, not as a part of applied mathematics, but as a part of pure mathematics, and in so doing, the theory will doubtless become more widely applicable than it has in the past. The application of the methods of pure mathematics to the theory of probability is its greatest hope of progress.

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[1] Jevons, *Principles of Science*, p. 206.

[2] *Ibid.*, p. 208.

[3] *Probability*, p. 366.

[4] *Ibid.*, p. 51.

CHAPTER XIII

THE ETERNITY OF TRUTH

§ 1. The supposed postulates of induction, the uniformity of nature and the law of universal causation, have been perhaps the greatest stumbling-blocks to the theory of induction; the difficulties they present must now be faced. Ever since the time of Hume, it has been recognized that the uniformity of nature is one of the important presuppositions of all inductive proof. Hume uses as his chief argument in demolishing the idea of necessary causation the failure of philosophers to prove the truth of that principle. We have found in the past that when one billiard ball hits another, the second moves; Hume asks, What right have we to assert that when the one event happens *in the future*, the second event will follow? He replies that our expectation is merely a habit, that we can give no demonstrative proof of our expectation. It is quite conceivable that the future might be different from the past; the uniformity of nature is consequently not a necessary truth. We must now ask ourselves what proof there is for this important principle.

Our first difficulty is the vagueness of the principle itself. In its original form, "Nature repeats herself," meaning that historical events recur in cycles, it is quite clear and familiar as an assumption of some ancient philosophies, especially of Stoicism. But modern statements of the principle, in avoiding such an implication, are not clear. Hume's formulation "that like objects, placed in like circumstances, will always produce like effects"[1] is open to the objection that Hume gives us no way to determine whether two objects are really alike or not. As a matter of fact, when two objects act differently, we conclude

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[1] *Treatise*, Bk. I, Pt. III, sec. viii, p. 105.

ipso facto that they are different. Hume's statement is thus thoroughly circular, neither provable nor disprovable, if it is intended as a scientific statement of the principle. The conception of the uniformity of nature has appeared so self-evident that logicians have only recently seen the vagueness of the concept and have attempted to state it clearly.[1]

A little consideration will show that the uniformity of nature does not mean that nature always acts in the same way. Venn[2] has shown, in an instructive fashion, that all the laws of nature may still continue true and yet there be no uniformity of experience at all—merely send the earth on a hyperbolic path, so that there would no longer be any seasons, day and night, etc., and no event would recur. Then the uniformity of nature does not necessarily mean that there are any repetitions of the same phenomenon. Conditions may constantly change, so that events do not recur, but natural laws may nevertheless still hold true of these changing phenomena.

While it would be much more difficult for human beings to discover any natural laws under such continually changing conditions, it is by no means certain that it would be impossible. There could be no empirical laws, because empirical generalizations are merely statements of uniformities of experience, but it might be possible to discover and prove rational laws.

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[1] The most significant recent statements are those of Broad (*Mind*, January, 1920) and Keynes (*Treatise on Probability*, p. 226). Broad recognizes the extreme vagueness of the conception of a "natural kind," and that the ultimate criterion as to whether two substances belong to the same "kind" is whether they act alike. Keynes writes, "The law of the Uniformity of Nature appears to me to amount to an assertion that an analogy which is perfect, except that mere differences of position in time and space are treated as irrelevant, is a valid basis for a generalization." It is "a generalized judgment of irrelevance" (*ibid.*, p. 226). To this Richtie has justly objected that "we can never know whether two events differ merely in respect to spatio-temporal poistion, because all we observe is the configuration and motion of material bodies" (*Mind*, July, 1926, p. 313).

[2] *Empirical Logic*, chap. iv, p. 98.

Such a law as the law of gravitation does not assert that, under certain conditions only, bodies attract each other with a given fixed force, but that, under *all* conditions, masses attract each other with a force which varies under different conditions in accordance with a certain mathematical formula, so that, given the conditions, the force can be determined. It is quite conceivable that in a continuously changing world some genius might guess the law of gravitation, and, by means of careful records of past events, prove its truth. He would even be aided in that he would not need laboriously to vary conditions, but they would be constantly varied for him so that a wide range of verification would be easy! The reason that experiments and observations are repeated is because we are not sure that accurate and careful immediate descriptions have been given, so we repeat the process; if a means were discovered of getting exact and accurate records of experiments and observations, together with all the conditions under which the event occurred, it would not be necessary to repeat any experiment or observation.

The uniformity of nature does not presuppose any phenomenal uniformity of occurrence. Once it is recognized that the empirical law is not the only form of inductive generalization, uniformities cease to be important. It is not even true that mere spatial differences make no difference in phenomena. Pendulums do not beat the same on top of a mountain as at the bottom of a mine, and yet the uniformity of nature is not contradicted thereby.

It is difficult or impossible to conceive in detail a world in which the uniformity of nature is not obeyed—for the reason that we have had no experience of a non-uniform world. Babbage's calculating machine, which works invariably during a finite period and then manifests a single breach of law,[1] is not an example of the breach of the uniformity of nature, but of its obedience—most calculating machines act that way; after operating correctly for a long period of time they begin to make mistakes! Yet such a mistake is held to be in accordance with the uniformity of nature.

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[1] *Ninth Bridgewater Treatise*, p. 140; quoted in Jevons, *Principles of Science*, xxxi, p. 743.

The uniformity of nature does not require the continued existence of any objects. It does not require the unaltered existence of our globe or of human life.[1] It is quite conceivable that the earth might collide with another heavenly body and all living beings disappear, without a single law of nature being changed or broken. Even the laws of the behavior of living creatures would continue as true as before, though there would be no living creatures. The laws of the emission of radioactive energy will continue true (though inoperative) after the last radioactive substance has decomposed, since these laws merely assert that *if* there are radioactive substances, they give off energy in a certain manner. Natural laws do not require the continued existence of their objects to be true.

§ 2. What, then, does the uniformity of nature mean, since it does not assume any phenomenal uniformity nor the existence of any objects? It merely asserts that *all truths*[2] *are eternal*—that when we have once found a rationally established law, we have found something that is somehow inherent in the nature of the universe in such a way that it will forever remain true. This principle might well be designated as the *eternity of truth* instead of the uniformity of nature, provided that it is remembered that all truth is objective and independent of our discovery of it—it is proved true because it is verified in experience, not because it merely obeys such subjective criteria as self-consistency, etc. The empirical verification of such a principle as this would merely require that we should be able to tell when a law is true and when it is broken—surely no impossible demand.

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[1] Cf. Jevons, *op. cit.*, pp. 745–49.

[2] By truth is meant all those propositions that have or will become knowledge to any being. Truth is a more inclusive concept than knowledge. Knowledge is dependent on the progress of human science; truth is that which becomes knowledge.

A corollary to the foregoing principle is the *indifference of space*—that differences of location in absolute space do not affect any event. The earth and astronomical bodies continually move into new places in space; if truth is eternal, absolute location in space can make no difference in the behavior of bodies. A part of every law is the statement of the conditions under which it is true—under the same conditions we expect the same phenomena (an alternative statement of the eternity of truth). The principle of the indifference of space accordingly asserts that location in absolute space can never be a part of the conditions to any law.

Differences in relative time do affect events: one billiard ball can hit another only at a certain interval after it starts moving, no matter how close together the balls are placed—all physical and chemical phenomena involve relative time; the eternity of truth asserts that the absolute time at which movement occurs does not affect that movement. Differences in relative space likewise affect phenomena: a magnet attracts iron with a force which varies with the square of the relative distances between the two—yet no account need be taken of absolute space; under the same conditions the attraction is the same anywhere, provided there is no significant relative spatial difference. No matter whether the time of an event is present, past, or future, or whether its location is here or there, given the same conditions, we expect the same consequences. These principles accordingly assert that absolute time or space cannot affect any event.

§ 3. Such principles as these, if proved true, would not make any merely empirical laws more certain than before. Empirical laws are statements of experienced uniformities, all cases of which may not have been observed, and which therefore may have to be restricted to those cases which have been observed. Such empirical laws are not deduced from any further law. Our ignorance of the conditions for their truth prevents us from knowing when they will be true and when false, and so we dare not infer with certainty from one occurrence to another. The eternal truth of such an empirical law would merely mean that under some unknown set of conditions that known event will recur—since the conditions cannot be specified more than very approximately, we can never be sure whether the event will occur again or not. For example, we know that cold hardens and toughens metals—but, after a certain point of cooling, metals become brittle! Our ignorance of the conditions of molecular cohesion prevents us from knowing or proving anything more than an empirical law in this case. The principles of the eternity of truth and the indifference of space would not help to make the empirical law that cold hardens metals universally true, because we cannot say when it is true and when not.

§ 4. The eternity of truth is a presupposition of deductive as well as of inductive logic. What right has anyone to speak of "eternal truths" in reference to deduction any more than in induction? How can we be sure that, for example, the law of identity, *A* is *A*, is eternally true? The very fact that there is such a law implies that it is thinkable for it to be otherwise. Fichte showed that the eternal truth of even this law presupposes an eternal sameness in the universe.[1] If, in Hume's "sifting humour," we question the existence of such a sameness, we no longer find any justification for the eternal truth of *A* is *A* or of any other proposition. How can we prove that at some time in the future the proposition, *A* is not *A*, will not take the place of the other? We cannot prove it any more than we can prove the eternal truth of the law of gravitation, for we cannot be sure that our concepts will remain the same, even while we pass from subject to predicate. How do we know that the laws of thought are more permanent than any other scientific laws? Suppose human minds were no longer able to think in accordance with the present canons of deduction—how altered our whole fabric of mathematical and deductive principles would become! Even pure mathematics would become gibberish and would be utterly changed. Not simply induction, but all reasoning, assumes the permanence of its product, the eternity of truth.

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[1] Cf. chap. ii, § 7.

The man who upholds the possibility of knowledge is consequently at a great dialectical advantage over his opponent; for his opponent, to argue at all, must assume the very principle of the eternity of truth that he is denying. Unless he does make that assumption, he has no guaranty that *his* arguments will continue to remain true! The skeptic is thus put into the position of the solipsist—if he attempts to express his skeptical opinions, he refutes himself unless he is willing to admit that at the next instant all may be changed and his arguments become valueless. Thus the possibility of any lasting disproof to the permanence of truth is disproved.

§ 5. Since the eternity of truth is thus the presupposition of all reasoning, the proof of this principle becomes of fundamental importance for all philosophy. Unfortunately for philosophy and science, it must be confessed that at present the permanence of truth is merely an empirical statement. We are merely able to say that what truth has been discovered in the past is still true; we cannot say *why* it has remained true, nor be absolutely sure that it will continue to remain so, since no merely empirical law can attain to certainty. Consequently we have to confess that our armor of defense against skepticism is not complete, for this empirical element is mixed in with all knowledge. In the present state of philosophy, knowledge still has its Achilles' heel.

This does not mean that we are plunged into Humean skepticism. The cogency of Hume's argument lay in the assumption that the method of enumeration, by which an empirical generalization is established, is the *only* method of induction, and consequently, that the uniformity of nature cannot, because of the nature of logical method, ever be established. Once it is recognized that rational induction is different in kind from empirical induction, the cogency of Hume's argument disappears; for there is no reason that the principle of the eternity of truth should not become a rational and certain truth, instead of being merely an empirical law. The progress of science consists in changing what have been merely empirical uniformities into cases of more general rational laws. The failure of logicians to understand the difference between these two kinds of induction and their failure to appreciate the possibilities of rational induction have prevented philosophers from attempting to establish the permanence of truth by rational methods. Once this possibility is seen, we may expect that succeeding philosophers will perceive the rational law of which this principle is an expression, and demonstrate its truth. While the skeptic may still say, "Your armor is not invulnerable," we may nevertheless reply, "Only at one point, and the means for remedying that defect are at hand. You can no longer hold that this weakness is inevitable. We can show that it is possible to mend it, and to make ourselves invincible, though we cannot achieve that state at present."

For it by no means follows that because a law is only at present susceptible of empirical proof, it must remain so. More often than not empirical laws have been proved to be true—that is the reason we put so much reliance upon merely empirical laws. The succession of day and night was merely an empirical law until Copernicus and Newton bound it up with physical law in general, and established its necessity. The conservation of energy is still an empirical law, and it may be long before it can be rationally established—yet that fact does not prevent men from having confidence in it. The principle of the eternity of truth is, moreover, the best established of all empirical laws. "Hard" indeterminists even challenge the truth of the principle of the conservation of energy for the sake of moral responsibility—but there is no reason for suspecting the permanence of truth. It is only the speculative philosopher who has ever stopped to challenge it, and then even he has to confess that for practical life and for all reasoning he has to assume the truth of the very principle he challenges. The only reason that he can allege for his challenge is that no proof of its undeniable certainty has as yet appeared! Yet the sciences are all of them built upon this principle; they have been and are successfully dealing with nature on its assumption. Lyell built up modern geology on the thesis that the forces operating on and within the earth during the past are the same as those today; astronomy carries us back to a much greater past than even geologic time, and shows us the continued truth of our most recently discovered scientific law for the past billions upon billions of years, as far back as we can go. The fact that the eternity of truth is still only an empirical generalization does not throw any doubt upon its possible truth.

Just how this principle is to be made a rational truth is not at present apparent. The relationship between an empirical fact and its rational generalization is obscure until the genius perceives it in a flash of intuition. The permanence of truth seems to be closely connected with the relativity of time—the knife-edge present is a highly anthropomorphic conception; since all time is relative to a present which is itself relative to the observer, it may follow that all objective truth is non-relative and eternal. Or a metaphysical proof for the existence of a good God may give as a consequence the eternity of that truth which is His being. Unfortunately these conceptions are as yet mere speculations and need logical substantiation. The solution to the problem of the rational proof for the eternity of truth will come from metaphysics.

It is nevertheless important to realize that the dubiety of this principle does not inhere in our method of proof. It was here that the cutting point of Humean skepticism lay—Hume showed that the logical methodology of empirical induction requires the making of a metaphysical assumption, the uniformity of nature, in order to produce any knowledge. But the rational inductive method does not necessarily make any such assumption. It is inherent in the nature of that method that the permanence of truth *might* be logically established, if only we had the material for doing so. Therefore we escape from Hume's serious methodological difficulty; the demonstration of the eternity of truth becomes for us a merely academic problem.

There is, then, every reason to consider the principle of the permanence of truth as true, and any serious doubts of it as childish, although we recognize that it is not yet proved beyond the possibility of a doubt. We have an inestimable advantage over our predecessors, in that we have a method whereby we could prove its certain truth, although we are as yet unable to apply that method. We may then proceed to our reconstruction of philosophy, confident that we are making no mistake in trusting ourselves to this principle.

§ 6. The other of the two supposed postulates of induction, the law of universal causation, that every event has a cause, is not necessary to the method of rational induction. This method requires only deductions from cause to effect, whereas the law of universal causation becomes a necessary assumption only when we need to make our deductions carry us in the reverse direction—when from an effect we attempt to deduce a cause—as when Berkeley attempted to argue from the existence of ideas to the existence of a necessary Cause. Rational induction does not proceed in this fashion; it deduces the consequences of a causal hypothesis and shows the truth of that hypothesis by the unique correspondence of its consequences with experienced facts; it does not reverse the process of deduction.

Consequently, we do not need to assert the truth of universal causation or its consequence, universal determinism. For rational induction, causal connection is limited to those cases in which it has been found. Moreover, the eternity of truth requires us to believe that where a causal law has once been found, it is permanent. Outside of those cases in which a causal connection has been proved to exist, it is quite possible that universal causation may indeed exist; we may suspect an invariable causal connection and form hypotheses which make that assumption; but, until such a hypothesis is demonstrated to be true, our theory of logical method does not require us to assume any causal connection in any previously undetermined case. The method of rational induction does not, therefore, prejudge the controversy of determinism with indeterminism, since this method merely requires the eternal existence of causal connections under those conditions that such causal connections have been found—a view that is accepted by most advocates of free will. As a logical method, rational induction would be capable of establishing the correctness of either side of that controversy, providing the requisite facts and deductions were furnished. The impartiality of this method toward metaphysical problems ought to commend it to disinterested thinkers.

CHAPTER XIV

THE TRANSCENDENCE OF EXPERIENCE

§ 1. It is very natural for the cautious thinker to refuse to accept anything as true unless he is compelled to do so. A critical attitude is part of the very essence of philosophy and of all science. Aided by other influences, this attitude has flowered into the phenomenalistic spirit shown in much of recent science and philosophy, which refuses to go beyond immediate experience. The ideal of science has been held to be simply "the description of the course of events by the aid of the fewest and simplest general formulae. *Why* things happen as they do, it is now said, is no proper question for science; its sole business is to enable us to calculate how they will happen."[1] "Atoms," "forces," "ethers," and "sensations" are said to be just as much symbolic creations of our imagination as the "imaginary quantities" of our mathematics.[2] This phenomenalistic account of science has sometimes been taken advantage of by the philosopher to puff his own science, so that natural science is said merely to describe the facts (give formulas for purposes of calculation), but philosophy to interpret them (give an account of things as they really are).[3]

In addition to the philosophic attitude of criticism toward any unusual theory, there have been two streams of influence which have been chiefly responsible for the form this phenomenalism has taken—the empiricism of Hume and the idealism of Kant. In order to understand and evaluate the basic assumptions upon which phenomenalism rests, it will be well to remind ourselves of the logical positions held by these two thinkers.

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[1] A. E. Taylor, *Elements of Metaphysics*, p. 174.

[2] *Ibid.*, p. 192.

[3] *Ibid.*, p. 193.

For Hume, induction consists solely in the establishment of what we have called empirical laws—statements of observed uniformities. It follows, as a natural consequence of that view, that no statement of a uniformity can be more than an account of what has happened and may happen again. I repeatedly see one billiard ball hit another, and the second one move—my empirical law can only describe the events which I have observed and their uniform succession—I have only observed that when one ball hits another, the second moves. No "force" has been perceived, and hence no "force" can be described, so that if my empirical formula contains the concept of a "force," the introduction of that concept is illegitimate—for an empirical law is merely a description of what has been uniformly observed. Hence the function of science is not explanation, but description only. Phenomenalism is the natural consequence of this view. Wherever this empirical conception of induction is held, phenomenalism will be the natural result. When once we appreciate the distinction between rational and empirical induction, we recognize that Hume misunderstood and greatly oversimplified the procedure of science. His argument holds only for empirical induction, not for the rational establishment of inductive laws.

Kant reached a phenomenalistic conclusion on other grounds. For him, as for rationalists and empiricists alike, the only method of proof consisted in deductive demonstration. Hume's criticism had bankrupted deductive demonstration in philosophy, since it showed that there was no foundation for the concepts upon which the whole of metaphysics rested—the causal axiom, the concept of substance, and the like. Kant sought to re-establish these concepts as valid for deduction, and found a basis for them in the characteristics of the transcendental mind, whose forms and categories are inevitably found in all knowledge, because knowledge is constructed by the transcendental faculties from the manifold of experience. Consequently, if the concepts of causality, substance, etc., are contributed to experience by the mind, their use must be limited to that experience in which they are found, and they cannot be applied to anything outside that experience; Kant restored metaphysics by limiting it strictly to experience and excluding the knowledge of any "thing in itself." The result of the Kantian restoration of metaphysics was to limit all knowledge to the realm of "possible experience" where the transcendental faculties could do their work of supplying the categories, without which knowledge is impossible. While many of Kant's followers rejected his realm of things-in-themselves, they retained his conception of experience as created by the mind—and hence adopted the phenomenalistic view that knowledge is possible only within the sphere of experience.

Thus the two great masters of modern philosophy, Hume and Kant, both developed a phenomenalistic metaphysics. The consequence has been that, until very recently, when some of the realists have dared to attempt the overthrowal of even Kant's philosophy, the possibility of transcending experience has been uniformly denied.[1] It is asserted that between experience and what is outside experience there is an impassable gap; and that, if that sort of a gap is admitted to exist, no logical knowledge of anything that is outside of experience is possible.

Now the statement that experience cannot be transcended is highly ambiguous, for the reason that the term "experience" needs definition. The foregoing statement may mean that it is *my* experience which cannot be transcended, i.e., that I cannot know anything which has not come into my own individual experience. Or it may mean that all *human* experience cannot be transcended, i.e., the experience which human beings have had up to this moment. Or it may mean that it is all *possible* experience which cannot be transcended, i.e., all the experience which all beings of the sort that I am can have at any time, either past or future. The solipsist and the phenomenalist (when logical) stick to the first interpretation, and the Kantian to the third. We must discuss them in turn.

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[1] Cf. J. B. Pratt, "The Possibility of Knowledge," in *Essays in Critical Realism*, for a modern defense of transcendence.

§ 2. First, however, we should observe that all knowledge of any sort involves some sort of transcendence. Even what we have called "description," in its strictest sense, transcends its object. The exact description given by an introspecting psychologist or practiced observer is not the experience that he is describing—it is *distinct from* his experience, though it refers to his experience. The experiencing itself we cannot call knowledge—it is direct acquaintance with, but not knowledge of, the object. The experiencing can only become knowledge when it clothes itself in concepts, and hence is *susceptible of logical criticism*, i.e., when it becomes other than itself. This interpretation of knowledge so as to exclude all merely psychological perception, confining it to what is susceptible of logical manipulation, is a consequence of our view that knowledge and opinion are logically distinct. Compulsory power over an experienced mind can only be had by that which is amenable to logical testing. While the word "knowledge" is frequently used of direct acquaintance, such a meaning is illegitimate in logic, since it asserts a sort of knowledge with which logic has nothing to do. Unless "knowledge" is used in this purely psychological sense, it is always *knowledge about*, and never *knowledge with*, that which it concerns. Therefore, all knowledge, in the logical sense of the word, transcends itself and refers to what is literally other than itself. Any epistemological theory that denies transcendence of experience must take this fact into account.

Transcendence of itself is thus one of the fundamental characteristics of all knowledge. The problem is, How far can this self-transcendence go? As far as my experience? Or to all human experience? Or to possible experience? Or beyond that?

Is it possible to transcend the bounds of my own experience? It seems difficult to doubt that such is possible. My friend tells me over the long-distance telephone that he sent a letter by mail. That mailing of the letter is an event which was inaccessible to my perception, so I cannot know it directly. But when I receive the letter in the post, with the postmark of the place where he was, stamped at about the hour which it should bear, I conclude that the hypothesis that he sent me a letter by mail is sufficiently widely verified to justify me in accepting it. Not only practical men, but such a science as history, continually transcend the experience of the individual in its formulations. It may have been impossible for me to perceive Julius Caesar, but there is sufficient evidence of the most varying nature to justify me in accepting as completely verified the hypothesis that he lived. When we have accepted this possibility, we have already gone farther than Hume's account of empirical knowledge would permit us.

Can all actual experience be transcended? Here, again, we must answer that it is possible. I go into a primeval forest and see a log rotting on the ground. Can I not then conclude that it grew from a shrub to be a tree at that place, flourished, and fell? No human eye saw it before mine did; its growing and flourishing were beyond all human experience; and yet I can infer, by rational induction, its growing and flourishing, as surely as if I had been there watching it all the time. So in practical life, we do not rely upon direct testimony for all our knowledge, but go beyond it, in many cases by circumstantial testimony.

It may be more difficult to be certain in cases in which we have circumstantial evidence alone, but, given enough of it and of the right sort, certainty is quite possible. Indeed, we are oftentimes made more certain by indirect confirmations than by direct testimony alone. Among the natural sciences, geology depends almost entirely for its conclusions upon transcending human experience. Are we to say that we cannot be sure that the scratches upon a rock were made by a glacier, just because no human being was there to see? Or that the chalk with which I write was not once part of a marine creature living where now there is dry land? Hypothetical inference enables us to deal with events which happened long before any human consciousness appeared on this planet. Human experience is plainly transcendable, though, for Humean skepticism, such knowledge is quite unjustifiable, for such knowledge is plainly not a description of uniformities, but the assertion of unexperienced entities!

Can all possible experience be transcended? Thereupon the question arises as to whether we can say what is possible and what is not possible. Are we to assert the existence of a certain permanent structure of the mind or of the universe, which limits experience to certain possibilities? If so, how are we to experience that structure? Kant's forms and categories are certainly "arts hidden in the depths of the soul," which are beyond human experience just as truly as any thing-in-itself. A structure of the universe which limits the possibilities of human experience—how are we to experience that? Would not any knowledge of such a thing be likewise transcending all possible experience? It certainly is unexperiencable by any human beings at present—and if it might be experienced at some time in the future, how are we ever going to say what is possible and what is not possible in experience? The plausibility of the term "all possible experience" is due to its vagueness; as soon as we pin it down to one meaning, we find that such a limit can be overpassed.

A reasonable enough interpretation of "all possible experience" is that it is the sort of experience which human beings can or have had at any time, either past or future. That is more definite; it allows for the extension of experience by keener telescopes or finer microscopes, though of the same sort as what can be had now. But what then shall we say of the recent triumph of physics, in proving the existence of the electron? For a long time the atom was thought of as a figure of speech, useful no doubt, but not literally true. "The atom of chemistry is an 'Idea,' in the strict meaning Kant gave to this term—in so far as it possesses 'a most admirable and indispensably necessary regulative use.' "[1] But now a single electron, a body immensely smaller than an atom, has been caught upon a drop of oil and made to move up and down between electrically charged plates, so that its electric charge has been exactly measured. Professor Millikan, who first performed that experiment, asserts that the existence of the electron is as certain as the revolution of the earth. An electron is immensely small; it is so small that it is doubtful if it can subtend a ray of light, hence it can never be seen or come within the range of any human sense; it is literally a metaphysical entity, being beyond the range of all possible experience of any sort such as we know—yet physical science asserts that its existence is absolutely proved. Between the electron and anything that can come within the range of the human senses, no matter how they are sharpened, there is a tremendous gap, yet that gap has been bridged by the method of rational induction. The proof of the existence of the electron is not confined to this one experiment alone; a host of other phenomena of widely diverse sorts are explained by the existence of this minute entity. When, twentyfive years ago, Ostwald and other positivists among the physical chemists launched a determined attack upon the atom, they were merely following out the logic of their phenomenalism, for the atom is likewise a metaphysical entity, beyond the range of all possible experience. "The faith of physicists, however, was not for a moment shaken; and it has been justified by the progress of discovery in the intervening years. The last doubting Thomas has been convinced."[1] Thus the progress of the inductive sciences has driven even scientific phenomenalists to admit the existence of entities that are beyond the bounds of all possible experience.

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[1] Cassirer, *Substance and Function*, p. 210.

An objector says: These atoms and electrons are the sort of things which we could perceive if our senses were sharp enough —they are phenomenal. We reply: But the "essence" of the electron is electrical, and electricity cannot be perceived by any sense, how can it be phenomenal? Well, then, supposing we had another sense to which electricity would appear as a sense quality, and supposing that that sense were sharp enough to perceive minute quantities of electricity, and our other senses were sharp enough to perceive minute bits of matter, would not the electron then be a phenomenal object? It must be granted that such would be the case; but it should be observed that, in order to make the electron a phenomenal object, it is necessary to suppose an entirely new sense, and to magnify the powers of the other senses to such a degree that they, too, become practically new senses. Such a change in the powers of our senses is hardly greater than would be necessary to perceive any of the traditionally transcendent metaphysical objects—God or the soul. Surely no metaphysician would deny that if men had an appropriate sense-organ, and if that sense-organ were sufficiently keen, we might perceive God—the medieval mystics were sure that they had perceived Him—but the metaphysician thinks that he can set the bounds of all possibilities, and that such a sense-organ is impossible. For the mystic, all things are possible to God, and he would severely challenge the bounds that the metaphysician sets to the possibility of knowledge or of God's power. This dispute between the metaphysician and the mystic may well teach us how dangerous it is to attempt to state what are the limits of *possible* knowledge, and that however we make a distinction between *actual* knowledge and *possible* knowledge, we are on very dubious ground. While it may be conceded that no actual perception of these metaphysical objects has been *proved* to have occurred, yet we ought to remain strictly agnostic as to the possibility of such a perception. When we undertake to go beyond the bounds of actual knowledge to possible knowledge, we are asserting a boundary for knowledge which must necessarily remain very vague, precisely because we know nothing about it.

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[1] L. D. Woodruff, editor, *The Development of the Sciences*, p. 63.

The atom and the electron are metaphysical in any sense that God or the soul are metaphysical. If the atom and the electron are phenomenal, then the word "phenomenal" is used to mean merely "really existent," and God or the soul may be phenomenal. The atom and the electron have never been experienced, they cannot now be experienced, and very likely can never be experienced—yet scientists are certain of their existence, and the acceptance of their existence has gained ground steadily until today physical scientists are practically agreed upon it. What surer proof can there be that experience can be transcended?

On the other hand, if we say that the electron comes within the field of "possible experience," what can be excluded from that field? If we make "possible experience" a synonym for "that which can be conceived," a restriction of knowledge to "what can be conceived" is meaningless, for nothing that can be thought about is outside of that field. If whatever can be conceived can be known, then God or the soul might be known, and we must admit the possibility of what is transcending experience in any intelligible sense of the word.

Or shall we say that the electron, because its perception transcends experience, cannot be proved to exist, that it is merely a term summing up certain experiences? Then we must go on to hold that all similar scientific discoveries which assert the existence of unexperienceable objects or events are likewise fictions. Bertrand Russell accordingly asserts that the view that the earth goes around the sun is merely a pictorial name for a certain mathematical expression.[1] Then he must also agree that the approximate sphericity of the earth is another merely mathematical expression. We certainly never see or feel the earth as globular; we conclude that it has that shape as the result of such phenomena as circumnavigation, circular shadows of the earth on the moon, etc. The rotundity of the earth is literally beyond human experience. Consequently phenomenalism must hold that we can never know that the earth is spherical; its sphericity is a mathematical fiction. If then to Mr. Russell the sphericity of the earth would be a fiction, what must he say is the shape of a large stone sphere, so large that one's arms cannot reach around it to feel all sides of it at once, so that one can only walk around it? Is it spherical? One sees it as circular; but its sphericity is beyond anyone's possible experience. Then must we say that it is not spherical, that its sphericity is only a mathematical and fictitious way of stating its properties? That when a ball is small enough so that one can reach all around it at once, it is spherical, because that shape is felt; but that as soon as a ball ceases to be small, it cannot be spherical?

Consistent phenomenalism must accept all of these absurdities and many more, because it accepts only immediate description as a correct logical method, and immediate description does not permit the assertion of any rational generalizations whatsoever. A phenomenalism that refuses to go beyond experience must, moreover, find the perceptual judgment inexplicable.[1] It cannot assert the existence of any objects at all; it can only record experiences, of whose cause nothing can be known. It must accept solipsism, for one can never experience any other person directly and consciously; one can only perceive his voice, his appearance, etc. It cannot even assert any probabilities, for a statement of probability can only be founded on some knowledge of the sort that consistent phenomenalism does not allow.[2] It cannot consistently act, for it has neither sure knowledge nor even probable "knowledge" of the future, so that no plan of action is more reliable than any other. It can only justify death by inanition—unless one is willing to be inconsistent and act *as if* the objects and knowledge that it denies really do exist. Phenomenalism when consistently and thoroughly carried out, refutes itself by its absurdities.

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[1] In *Whither Mankind*, edited by Charles A. Beard, p. 77.

Modern science, in every one of its rational generalizations, has achieved the transcendence of experience that phenomenalism denies. The revolution of the earth is not as apparent a contradiction of phenomenalism as the electron is, because it is a more familiar conception, and because it is easier to imagine one's self seated on a star watching the earth move; but, since such a transcendence of earthly limitations is nevertheless impossible, the knowledge of the movement of the earth is itself a transcendence of all possible experience. The theory of biological evolution is likewise a case of transcendence of all possible experience—no human being could possibly have been present to witness the steps of development by which human beings came into existence.

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[1] Cf. chap. xi, §§ 3, 4.

[2] Cf. chap. xv, § 3.

One of the most striking examples of transcendence is the spectroscopic evidence for the existence of the same chemical elements in the sun as on the earth. Jevons has quoted Kirchoff's argument from probability for the existence of iron in the sun.[1] But the proof of the fact of chemical continuity between the sun and earth does not depend simply upon probability. Spectroscopic lines have been found to go in groups, and so the occurrence of the sixty lines of iron very likely ought not to be considered as sixty independent events. For example, there are found in the spectrum of some suns a homogeneous set of lines, called the Pickering lines, similar to those of hydrogen, but occupying a slightly different range. At first it was supposed that they were due to hydrogen, but later it was found that ionized helium would produce those lines. Under the immensely high temperatures and electrical conditions in some suns, it is to be expected that helium would exist in an ionized condition which is exceptional or artificial on the earth. Hence the discovery of the spectrum of *ionized* helium in a sun gives a wider confirmation to the hypothesis of the existence of common chemical elements than just that of ordinary helium would do, since that phenomenon shows that these elements behave in that sun in the way in which we should expect them to behave under the conditions of temperature and excitement known to exist there.

Furthermore, it is to be expected that the lighter elements would be found at the surface and the heavier ones at deeper levels. If the surface of the sun's corona were found to give the spectra of the heavier metals, and deeper levels the lighter gases, we could well be suspicious of any spectroscopic analysis of the chemical structure of the sun, regardless of how many lines coincided. But the upper layers show the spectra of the lighter gases, and deeper layers those of heavier substances, just as the hypothesis predicts. So the proof for the community of chemical composition between the sun and earth is by a wide variety of verifications of this hypothesis. There is even an exception, the supposed "coronium," supposedly a gas unknown on the earth; but, in view of the range of verification of the hypothesis, this exception may be said to be, in all likelihood, only an apparent exception. In the case of the stars, such a large variety of verifications is not always possible; but it is found that the giant stars are composed chiefly of the lighter gases, and that in the denser stars the heavier elements abound. In this way, by what Whewell would have called a "consilience of inductions," the astronomer is able to transcend earthly experience, and determine the chemical constitution of distant universes with certainty.

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[1] *Principles of Science*, pp. 244 ff.

The transcendence of experience is, then, an accomplished fact and not a mere dream of unphilosophic "practical" men. We thus see that the goal of science is not mere description, but explanation, and, if possible, the discovery and statement of qualities of object which are inaccessible to any sense. Natural science, as the phenomenalist conceives of it, as a "system of thought which assumes nothing beyond the content of observed facts" is something "which never did exist, and never will."[1]

The reason that scientific phenomenalism has had such a vogue is that it developed during the youth of most of the sciences. Description is a much easier task than explanation, so every science began as a description of uniformities or empirical generalizations, and only gradually went on to rational inductions. Phenomenalism has served its function as a reply to medieval dogmatism, but it is no longer tenable, either as a factual description of the sciences or as a metaphysically established epistemological theory.

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[1] Huxley, *Lay Sermons, etc.*, "The Scientific Aspect of Positivism," p. 161, note.

§ 3. Then how is the transcendence of experience achieved? How is the gap bridged? An analysis of any one of the foregoing illustrations will show how it is done. It is the function of rigorous deductive inference to bridge the gap between the unknown and the known, but deduction must be supported by hypothesis and verification. We infer from the unknown to the known and then back again. We make a hypothesis of the existence of an unknown object, such as hydrogen in the sun; if that unknown object has any meaning at all, we can infer from the characteristics of that object various deductions as to what we ought to find in experience; then we go to experience and search for these predicted experiences. If we find a sufficiently large variety of them, we conclude that the object exists.

It is obvious that this wide deduction and verification is only possible in cases in which our unknown object is well defined, i.e., in which it has a large number of qualities, or in which it acts in a large number of ways, for it is the consilience of diverse results that ultimately establishes the truth of our hypothesis. The reason for our confidence in this method of transcending experience is that it has proved itself trustworthy as a matter of experience—in so many cases it has proved the existence of unknown objects or facts that were afterwards brought into sense experience. The detective has confidence in his means of unraveling mysterious crimes because in those cases in which a sufficient variety of pertinent evidence has been gathered to substantiate his theory, and in which eye-witnesses or the confession of the principals has subsequently been obtained, they have confirmed the results of his method of induction. In science there have been many cases such as Harvey's proof of the circulation of the blood, which was afterward confirmed when Malpighi could perceive, through the microscope, the capillary circulation that was beyond the experience of Harvey. Indeed, in the present state of science, most new and important hypotheses ought to transcend all past experience and predict something previously unknown which can be verified by observation or experiment. Knowledge always is other than, and so transcends, its object; the function of deduction is to proceed from the known to the unknown; hence it should not be surprising that it is possible to transcend experience.

§ 4. It thus becomes impossible for us to set any accurate limits to knowledge by saying that it cannot transcend experience. There are, however, certain limits that do vaguely mark boundaries which knowledge cannot go beyond. An object which is literally inconceivable, which cannot be thought by any twist or turn of the human mind, is plainly unknowable, for no hypothesis can be framed concerning it.[1] If God can be only described as the Upanishads describe him, by "*Neti, neti*," he is unknowable. The limits of knowledge are the limits of speculation.

It is, however, very difficult to say how far this literal inconceivability will limit knowledge; for, if the object produces any effects that come within experience, however remote, it can be known through these effects. For example, electricity, for which there is no sense-organ, has now become one of the fundamental characteristics of matter. Gravitational action at a distance was considered inconceivable until Newton made it a commonplace of thought. When no hypothesis can be conceived or when there are no effects of a hypothesis to be found within experience, knowledge must be impossible. But if everything in the universe ultimately affects everything else, nothing is unknowable. Science is ever increasing the scope of experience by its instruments of precision, and the human mind seems infinitely flexible.

A still more important limitation of knowledge is that due to our inability to make deductions because of the indefiniteness of the hypothesis. It is this sort of indefiniteness that has prevented scientific knowledge of the existence of God—God has meant anything and everything, and consequently, nothing. God has been appealed to to help the religious man out of any and every abyss of ignorance, and has become such a protean being that no specific deduction is possible from the concept of God, because anything is conceived as possible to him. If by the Unlimited is meant the Indefinite, then He is unknowable. The appeal to miracles had at least this justification—it predicated a definite characteristic of God; consequently, it could be refuted. If God is worth anything to the religious man, he is no remote potentate, but enters into human life and alters it; if that is the case, there is no impossibility of God being scientifically known, provided we can frame a hypothesis which is sufficiently definite to permit of accurate deduction. Indeed, the principle of superimposition decidedly favors the possibility of verifying such a hypothesis.

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[1] Cf. chap. v, § 7.

§ 5. It is not to be supposed that every rational hypothesis implies the existence of an external object. Many, if not most, scientific hypotheses are concerned with laws—statements of ways of action. Others are concerned with qualities of objects, such as mass, voltage, evolution, which are plainly abstractions. Some only concern themselves with objects. It is therefore important to determine what are the characteristics of an object as distinct from an abstraction. An object, as known by rational induction, is a verified hypothesis that asserts a unified something which is a particular in the sense that it can be identified, but is unchanged by a mere change in space or time. This identification in the case of physical objects is by means of a localization in space or time, not necessarily by any individually different characteristics of the object.

It is to be noted that this definition of an object omits any reference to sense-perception. An object is not a "permanent possibility of sense-perception," for some scientific objects (such as electrons) produce no direct sense-perceptions. Nor is it true that "the ideal passes into the real when its perceptual equivalent is found,"[1] as modern positivists think. Even as far back as Descartes it was recognized that the real sun is different in its characteristics from the sun as known by sense-perception, so that there is no strict "equivalence" between the real and the perceptual. Although verification ultimately depends upon experience, the connections between the qualities of the object and the perceived experiences are often so tortuous and recondite that it needs all the resources of deduction to untangle them. Any criterion that is to be used in establishing or testing the existence of an object and that depends merely on sense-perception would make a rainbow or a mirage a real object, because in such cases there is a sense stimulation or perceptual equivalent. Such a theory would also have difficulties in avoiding the conclusion that a seen ghost is a real object, since here, too, there is a "seen" thing. But a rainbow, when taken as a rational hypothesis, fails to verify itself as an object; it disappears completely upon a mere change in space on the part of the observer, as does a mirage; whereas a ghost utterly fails to confirm its presence, since the hypothesis that a ghost is present does not enable the deduction of a sufficiently wide range of verifiable phenomena. An illusory object is a hypothetical construct which cannot be verified.

§ 6. In the foregoing exact sense of the term, the only physical objects at present known, which exactly conform to our definition of the term, are the ultimate particles of matter, whether they are electrons and protons or something still smaller, for they alone remain unchanged in space and time. These may be called *scientific objects*.

In addition, there are what may be called *conventional ob-*

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[1] K. Pearson, *The Grammar of Science*, p. 41, note.

jects, collections of these primary scientific objects arranged in such a fashion that the collection approximately fits our definition, so that for practical purposes they are taken as objects. A table remains sufficiently permanent through time to be considered as an object, though we know that it will ultimately decompose. Likewise, a tree or a mountain is conventionally regarded as an object, even though its limits are not rigidly determinable. The case of the tree or mountain is also peculiar in that it cannot be physically displaced in space, except in unusual cases, and, in such an event, movement may change it; nevertheless it is conceptually movable—it can be imagined to move from its place without changing, so it is considered to be an object; whereas the redness of a hat can hardly be conceived to move without carrying part of the hat with it, therefore it is not thought of as an object.

The foregoing are physical objects. We may consider any unified presentation to the mind as a *mental object*, as, for example, a centaur or a ghost. As to spiritual objects, none are as yet known, since rational induction has not been applied to that realm. It is to be observed that the "mind" appears to be more like a conventional object than a primary physical object, in that it does not remain the same through changes of time. It is consequently not surprising that some philosophers, such as the Atomists, should have conceived of the dissolution of the soul on the analogy of the dissolution of a physical conventional object.

§ 7. In addition to the scientific physical object and the conventional object, which is a complex of scientific physical objects, there is another sort of object, which is the object as we perceive it with all of its sense qualities, as, for example, the table with its figure, color, hardness, texture, etc. It is the existence of this sort of object that the ordinary uncritical perceptual judgment refers to and that common sense believes in. But the careful checking of such a perceptual hypothesis fails to result in a verification, since no justification is found for the attribution of certain characteristics, as, for example, color (in the sense of the perceived color), to even the conventional object. We must thus distinguish a fourth sort of object, the *apparent object* of common sense and of the ordinary perceptual judgment. In its logical nature it is a hypothesis that is somewhat successful in verifying itself, consequently it has been widely accepted by uncritical people of common sense, but it has not been completely successful and so must be prohibited from entering the realm of knowledge and real existence, although it has been widely useful in the past.

§ 8. With this interpretation of the nature of an object, it is possible to give an adequate interpretation of what it is that makes an object *one*—an explanation that presupposes a correct understanding of how an object comes to be. An object, such as a table, has an infinite number of appearances—a different appearance from every different position in space—and an indefinitely large number of apparent qualities. These different qualities may be unified by being referred to a common space, but not so the different appearances, for, although they are congruent, each seems to occupy its own space. This characteristic of the appearances of an object has led some realists, such as Bertrand Russell, to assume an infinite number of interlacing spaces corresponding to the different possible positions of an observer. Thus there result an infinite number of congruent objects, which yet seem to be one. This mysterious multiplicity in unity can be simply explained in that from *one hypothesis* of an object there can be deduced as many perspective appearances as there are positions taken. Mathematics has developed such deductions under the head of projective geometry; common sense and art under the head of perspective. Hence the unity of an object is due, not merely to its localization in a particular part of space, but to the fact that the various phenomena it presents may be developed from one hypothesis. The sameness or identity of an object through time can also be shown to consist in the possibility of taking the same hypothesis (or hypothetical construct) and using it again and again to account for (i.e., to deduce) different experiences. Thus the continuity and unity of an object is due to its being one verified hypothetical construct.

CHAPTER XV

KNOWLEDGE OF OTHER PERSONS AND OF THE EXTERNAL WORLD

§ 1. The logical method which we have outlined in the foregoing discussion enables us to demonstrate the existence of other selves or persons and of an external world, for it gives us a method of securing certain knowledge or truth by the verification of a hypothesis, and also provides the possibility of transcending experience. By this method, the truth of realism can be established. In general, the method of proving this thesis consists in making a hypothesis that this person or this object exists and then verifying this hypothesis by the same method as that used in verifying hypotheses in general.

For the proof of the existence of another person, there is no need to introduce any unverifiable hypothesis of immediate intuition or mystic continuity with that other. One merely makes a hypothesis that this person exists, or, in more popular language, that he is "real," and then draws from the hypothesis various deductions: such that if one gives him a stimulus, he will react; or that, if he is "real," one can get certain stimuli from him. Then one proceeds to try out various particular cases of these general conclusions: one may speak to him and receive a totally unexpected reply, or one may pinch him and find one's self flat on one's back with a painful sensation in one's head, or one may merely reach out one's arm in the direction of his hand and obtain pressure, temperature, and other sensations. If the experiences that one obtains agree in a sufficiently varied manner with those deduced from one's hypothesis, it is verified.

Only in unusual circumstances do we go very far in this process of verification—usually, visual sensations alone are considered, sometimes only auditory sensations are added—with the result that we are sometimes deceived into making hasty generalizations. When we realize that fact, we say that we have had a hallucination or illusion. It is only when we are alive to such a possibility (i.e., when we realize the existence of an alternative hypothesis and determine to make a rigid check) that we proceed to make other tests in order to eliminate other possibilities and really verify our hypothesis completely. This process of verification is too well known and too frequently used to merit elaboration; but it may be worth while to discuss some logical aspects of this process.

The first question that naturally arises is the basis on which is made the deduction from the existence of a person to the various reactions that are to be obtained. It is, of course, one's own experience of one's self. The very conception of a person is that of a being like one's self, who has a body and can consequently be grasped or seen, who can think and consequently can reply intelligently to questions (under most circumstances; the apparent exceptions to this deduction are explained by supplementary hypotheses that this person is an idiot, a foreigner, unconscious, etc.). Thus the hypothesis of the existence of a *person* contains within itself possibilities of deduction which have been originally justified by one's experience with one's self and then by previous experience with other persons.

§ 2. Persons, in their innermost nature, are said to be minds. What right have we to infer from one mind to another? It is held that the breaches between "thoughts belonging to different personal minds . . . . are the most absolute breaches in nature."[1] But are they? Cannot I tell what are the thoughts in my friend's mind while I am conversing with him? The closer friend he is, and the keener observer I am, the more I can discern his inmost reflections—if I can get him to reveal to me what they are, I can compare his revelations with my inferences and see that they tally. I know the motives of my friend a great deal better than the motions of the molecules in the chair on which I am sitting. Every conversation assumes that the breach between minds is not absolute. Otherwise no conversation could be carried on, and it would be impossible for me to understand what my friend is talking about. The psychological phenomena of suggestion show how slight the breach between minds may become. Without any words or explicit ideas, merely by muscular tensions and the (often subconscious) perception thereof, a wave of emotion may sweep through a crowd and each of its members may, at the same time, realize (if he reflects) that his emotion is also the emotion of the crowd. Do not these phenomena show that the breach between minds is often far less than that between a smith's hammer when it is first lifted to strike and the anvil on which it is to fall?

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[1] W. James, *Psychology*, p. 153.

Not only do we know the bare existence of other persons, but we come to distinguish persons whose minds and actions are different from our own—the color-blind, the imbecile, the crippled, those lacking one or more senses, etc. Here sympathetic insight and experience enable us to formulate exact hypotheses as to the sort of behavior these "queer" persons will make under various circumstances, and to verify our hypotheses.

It is true that between strangers (i.e., persons experience with whom is rare and in whose case deductions from hypotheses cannot be made with surety), or for careless or inexact observers, the barrier between minds may appear insurmountable; but, on the other hand, there are cases in which a person may truly be known by a friend far better than he knows himself—he has been making hasty generalizations about his own character, but his friend has not been so prejudiced, and consequently has reached a truer perception. The well-known paradox of the lover who is in love, not with his beloved, but with an ideal creation of his own which he assumes to be his beloved, is an illustration of the converse fallacy—prejudice has blinded him to his failure to verify his hypothesis as to the nature of his beloved. Such failures are not confined to hypotheses that deal with persons: the strenuous upholder of the phlogiston theory or the Ptolemaic hypothesis was equally blind. Indeed, many a man has aptly been said to be "in love with" his theory when he failed to realize the fallacy of hasty generalization which he was committing. The barrier between persons or minds is thus no greater and no less than between my mind and any other object. "Mind lies in the open field of experience . . . . so far as knowledge of it is concerned, as generally accessible, as free to all comers, as the motions of the stars or the civilization of cities."[1]

§ 3. The failure to understand the logical foundation of this simple hypothetical proof of the existence of other persons has led to many philosophical curiosities. Solipsism is the most important of these. Solipsism is the logical result of a theory which refuses to accept anything more than immediate description as giving knowledge.

If the solipsist accepts any other logical process than that of immediate description, such as the perceptual judgment, he immediately contradicts his fundamental position. The perceptual judgment is a rational induction; rational induction enables us to transcend experience, and hence to disprove solipsism. If the solipsist admits even the empirical law as giving an approximately or probably correct view of reality, he likewise contradicts himself, for an empirical law cannot have the slightest logical worth, not even probability, except on the supposition that it is a form of rational induction; the admission of even empirical induction implies the admission of rational induction with its transcending of all solipsistic limits of knowledge. The solipsist can only admit immediate description in his endeavor to be self-consistent.

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[1] R. W. Perry, *Present Philosophic Tendencies*, p. 273.

When the solipsist attempts so to restrict himself, he immediately finds himself confronted by the *de facto* impossibility of carrying through the program of immediate description. Language is lacking to express exactly what is perceived. Rectangular tables and upright posts are not immediately perceived, only their perspectives appear to sight; common sense, which is usually affected by the solipsist, substitutes for the immediate description the perceptual judgment, which the solipsist has no logical right to use, but which he cannot avoid using because existing words are unable to express what is immediately perceived. If he should create a new language to express his immediate descriptions, the consistent solipsist would nevertheless find it impossible to live, for he could not anticipate experience. Hunger would not be to him a desire for food, merely a pain to be described and catalogued; because his pain disappeared once before when he ate, he would have no right to infer that it would do so again, but could merely record the pain and its subsidence as a fact, any explanation of which would involve him in an illegitimate inference. Since immediate description gives no explanations, it forbids any prediction of the future and consequently any practical dealing with its world. All that the consistent solipsist can do is to sit and witness the kaleidoscope of experience, record its changes, and die from failure to adapt himself to his world. If he even so slightly adapts himself as to take hunger for a call for food, he is illogical. He cannot even justify himself by saying that past experience makes it probable that his hunger will go away when he partakes of food. Immediate description justifies no probabilities, but only a bare description of the past, which has nothing to do with the present or future. To Hume, all probabilities ultimately become so small as to disappear—probability cannot even be the guide of life unless some logical theory is accepted, which justifies the transcendence of at least past experience—the theory of rational induction. Solipsism either refutes itself or kills its consistent upholder.

§ 4. The various intuitional theories of knowledge of other minds are similar logical curiosities. While it may be true that I may in some degree or sense be the very thing I know, or that individuals may be included within other individuals,[1] such a hypothesis seems to be contradicted by ordinary experience; and, if true, it concerns that part of myself which is outside of ordinary observation—it transcends present experience. Hence it would require a far greater degree of evidence than has been offered for it. Furthermore, there are exceptions to the theory—intuition of others should give correct knowledge of them, yet our knowledge of our neighbors is often incorrect in many details—my trusted friend proves faithless; my neighbor unexpectedly turns upon me in wrath; a seemingly reasonable scientist turns spiritualist; all of which events should be impossible upon the supposition of intuitions of other selves, unless that intuition is so faulty as to make it utterly untrustworthy for logical purposes. Had logic been able to solve the problem presented by the ordinary man's conviction and the philosopher's necessity of believing in the existence of other persons, such theories would never have arisen.

§ 5. Once we have demonstrated the existence of other persons, there is available a powerful means of checking our hypotheses—by utilizing the experience of others as well as our own individual experience. While the same cause may not affect two people in exactly the same way, yet it is possible to make allowance for the difference of psychological makeup, and so to use the experience of different observers for checking the same law. Astronomers have done this in determining the personal equations of different observers, thus making seemingly divergent observations homologous. When only gross results are taken into consideration, such a careful consideration of all the factors and such a detailed description of the resultant phenomena due to differences in observers as are to be expected in an immediate description are not necessary. Different observers at different times can put themselves under the same conditions and get similar results (or results that are similar in proportion to their individual idiosyncrasies). A check of the verification of a hypothesis by another person is perhaps the most powerful means that we have of making sure of a rational induction.

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[1] J. Royce, *The World and the Individual*, II, 238.

The most important use of this method is the checking of immediate descriptions. While the logical process of verifying an immediate description consists in the comparison, by an individual, of his experience, with the immediate description, which is a part of that experience, and while no more than a single person need be assumed for such a verification, so that immediate description requires no social factor, nevertheless the results of such an immediate description are usually social in their nature, i.e., they are not only for the individual who made them, but for others too. The result is that while a particular immediate description may be an exact description of the experience had by the individual who made it, nevertheless others may not mean the same by the terms which are used in that description, so that it may be utterly misleading to others, though not to the author.[1] Since language is a means of communication, and so implies a society of more than one person, an immediate description ought to be couched in language which is such that others understand by it the same experience that the author does. The best method of checking this exactness of social language is for another person to seek the same experience and make an immediate description himself, i.e., for the experiment or observation to be repeated by another person. When it is not possible so to repeat an experiment or observation, the accuracy of an individual's immediate descriptions has to be determined by his general habits of using language, particularly language of that sort; since it has been found that individuals are rather uniform in their habits, it is usually possible to get a good check upon the social accuracy of an immediate description in this manner. Techniques for making these checks have been worked out by social scientists and others when it has been found necessary, but their logical significance has not always been recognized.

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[1] This is not, of course, the only fallacy in connection with immediate description. The maker of such a description may not make his own ideas clear, and so fail to compare his experience with his own concepts, or he may take one meaning of a concept at the time he makes the description, then later forget that meaning and give another meaning to his concept. These are failures of the in-

§ 6. A very interesting metaphysical question now arises: Since the existence of other persons can be demonstrated, what is the inner nature of those persons? Psychologists, who have usually been very much closer to the current of philosophic thought than other scientists, have been very much hampered in their attempt to determine the nature or existence of a metaphysical self by the fact that such a self is necessarily outside of experience. Even granting the assumption, previously unjustifiable by logic, that other persons are like myself, it has yet been uncertain whether introspection reveals my own true nature. Some psychologists have even, in a thoroughly phenomenalistic spirit, denied the existence of any conscious thoughts, because such conscious experience, when belonging to any other

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dividual in dealing with his hypothesis. The other failure, that of not comprehending the meaning put into words by another person, is a social failure, and so needs a social check.

than one's self, must be outside of one's experience—radical behaviorism is in part the outgrowth of phenomenalism. Other psychologists have gone contrary to the common belief in the impossibility of transcending experience, by assuming the existence of a subconsciousness or a coconsciousness, and sometimes of an unconscious self. While hypotheses which deal with such entities must be very carefully verified, and while we must recognize that most psychological hypotheses which deal with such entities have not been adequately verified, yet we can now see that there is no logical objection to going beyond experience and assuming even the existence of a metaphysical soul, *providing the evidence justifies us in so doing*. While a logical treatise such as this one may be excused from not going deeply into such a profound metaphysical and psychological problem, we may yet point out that the belief in a metaphysical soul is not so utterly unjustified as is asserted to be the case by many psychologists and philosophers who maintain the Humean and Kantian refusal to go beyond experience, and find inductive validity only in immediate description.

The unity of a conventional object, such as a chair or mountain, is certainly not to be found in the object itself, since it is clearly a complex, sometimes quite heterogeneous in nature; its unity is attributed to it by the mind itself, since it can be dealt with as a unity for purposes of action; yet when an introspective account is given of the mind, it seems to be merely a succession of conscious states. Since the unity is not from the object, whence does the unity of the conventional object come? Evidently there must be a great deal of unifying activity "hidden in the depths of"—shall we say—a non-phenomenal soul? Does the unity-giving characteristic of conscious experience imply a unitary substance? Psychology and metaphysics must co-operate in this problem, which is presented when we recognize the logical possibility of transcending experience.

§ 7. The demonstration of the existence of external objects is precisely similar in its nature to that of other selves—it is by forming a hypothesis, making a wide variety of deductions, and checking of those deductions with immediate experience. Philosophy has been in an anomalous position ever since the time of Berkeley—natural science has been dealing successfully with external objects and discovering their laws, which have proved to be so different from what was expected, although philosophy could not justify their existence; the man on the street has remained entirely unconvinced by the idealist's arguments for the nonexistence of an external world—he was using the method of hypothetical induction in his own rough way; hence dialectic failed to convince him, unless he could first be won to discard his method of hypothetical induction and use only that of immediate description. Consistent subjectivism failed to win the support of even a large group of philosophers—nevertheless the arguments of the subjectivist remained unrefuted, and philosophers were unable even to prove the existence of the external world with which natural scientists were dealing so successfully! Is it any wonder that the philosopher was looked at askance, and that a sharp division was made between philosophy, which should be the science of sciences, and science, so that even some philosophers denied that philosophy is a science?

It may seem a mystery that the existence of an object which itself never comes within the field of experience can be demonstrated. This mystery is dissipated when we remember that while no object can come within experience, its effects do come within experience, and that the effect of an object is never limited to merely one phenomenon, but that each object cooperates in producing many phenomena, so that by tracing the various possible causal chains from each of a set of possible antecedents, we find that most of the possible antecedents are unreal, since the evidence required to corroborate their existence fails to appear; whereas the causal chain from one antecedent results in the deduction of so many different and independently observed consequences that this antecedent can be the only one which fits the fact; in this fashion we arrive at an unambiguous antecedent for the set of effects we have experienced. We must remember that, if our physical theories are true, every physical particle is being continually influenced by every other one, and so in its own measure reflects the whole universe; consequently it is not surprising that, given sense-organs to select certain types of stimuli, to transform and magnify them, we can, by the exercise of sufficient ingenuity, reconstruct the external world with which we are surrounded.

§ 8. The logical significance of the attribute of existence, which has furnished a historic bone of contention between rationalists and empiricists, can now be determined. When the existence of an object is said to be scientifically demonstrated, the meaning of its existence consists in that the object enters into causal interactions in such a way that some of the effects of those interactions come within experience, and that it is consequently possible to frame a hypothesis of its existence and to verify it through the observation of a wide range of phenomena. The attribute of existence thus adds nothing, as Kant said, to the content of the object; it merely implies that the object is such that a certain logical process can be gone through with regard to that object. The proof of an object's existence is through the observed attributes or effects of the object, when those effects can be independently discovered in a wide variety of phenomena; that proof is not, however, itself any of those attributes. The ontological argument, which depends upon the assumption that existence is an attribute similar in kind to other attributes, fails of cogency, not because there is a gap between thought and things, but because the ontological argument does not provide the right sort of a bridge between objects and concepts.

§ 9. Another historic conflict with which we can now deal is that of the external existence of primary qualities. Berkeley showed that both primary and secondary qualities are alike and must be treated alike; consequently, modern realists have been inclined to assert both kinds of qualities of the real world. Berkeley's argument for the subjectivity of primary and secondary qualities remains, however, unshaken; the use of the inductive method by psychologists has added to the proof for that view rather than diminished it. If secondary qualities are subjective, primary qualities must be subjective, too—then what is known of the external object? Our answer is that neither primary nor secondary qualities belong to external objects, but only those qualities which are necessary to objects in order to establish the truth of a hypothesis about them. These qualities are precisely those that natural science attributes to its objects—similar in nature to the primary qualities, but not identical with them.

Locke's mistake consisted, not in asserting figure, impenetrability, number, etc., of his external objects, but in asserting that the figure, impenetrability, number, etc., in the objects is itself a sense quality. If his thought had been clearer, he would have consistently held that the external objects cannot themselves come into sensibility; that only their effects can do so; and that consequently those sense qualities which are perceived cannot be the qualities possessed by the objects themselves; they can only be others which are causally connected therewith. Locke's difficulty lay in the fact that in his time physical science was so undeveloped that had he adopted this view, he would have found himself almost totally ignorant of the nature of his objects; rather than accept ignorance, he gave to his objects a fictitious set of sense qualities, to the inevitable confusion and downfall of his realism. In his conception of the third sort of qualities or powers,[1] he was far nearer the truth.

As an illustration, let us take our old friend, the table. Its top appears to me as a trapezoid, or some other form of quadrilateral, depending on my position. I rarely or never perceive it as a rectangle. Locke would have to accept the primary quality as belonging to the table—which would strictly be that of a trapezoid, since that is the quality which has come within my conscious perception. Yet Locke and common sense do not accept the apparent figure, but say that the table top "really" has another figure—a rectangle—which figure is never or rarely seen. In other words, rectangularity is not a primary quality, since it is not seen; it is not a sense quality in this case at all, but a hypothetical unseen figure, which is adopted because from it can be deduced all the apparent configurations that are seen. It is thus not a primary quality, but a quite distinct quality, similar in nature to some primary qualities. The qualities possessed by the external world are not those found in sense experience, but others, sometimes analogous to sense qualities (as in the case of figure) and sometimes not analogous (as in the case of electrical properties). Perhaps they had better be called inductive qualities, in order to distinguish them from both primary and secondary qualities.

§ 10. If, then, we agree with the idealist that all sense qualities belong to the inner world of experience, what can we say is the nature of the objects existing in the external world? These "real" objects cannot be apparent objects, the tables and chairs we perceive, for apparent objects are sense-perceptions; nor can it be said that any objective correlates of these phenomenal chairs and tables exist as scientific objects in the strict sense of the word. These may be said to be apparent or conventional objects, but not real objects. For a chair is, by its very concept, a unitary thing; yet we can find no boundary between what we call an objective chair and its surroundings. Its figure is not exact, but the statistical result of multitudes of minute irregularities, and there is a continual interaction and interchange of particles going on between what we call an objective chair and its surroundings. Our minuter knowledge of the physical constitution of conventional things has failed to confirm the hypothesis that there are any unitary objects short of the ultimate particles of matter. One chair is no more one in itself than a roomful of chairs. A chair is unitary for human purposes only; apart from human purposes it is a heterogeneous collection of ultimate particles in continuous flux. What, then, are the "real" objects? They can only be the ultimate material particles or electrical fields, whose characteristic ways of acting are natural laws.

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[1] Locke, *Essay*, Bk. II, chap. viii, sec. 23.

Contemporary science, however, is not yet sure that it has reached the ultimate particles of matter. Electrons may be composed of still finer particles! Consequently, we have to confess our ignorance, and take electrons only provisionally as real objects, recognizing that, although their existence is demonstrated, yet we cannot be sure of their ultimateness until further scientific evidence is produced. When, in our present state of knowledge, we have to confess that we know so little of external objects, is it surprising that Locke was not ready to admit that primary qualities belonged only to the mental world, and to take what would have been a very paradoxical position in the condition of natural science in his day, that of asserting the existence of external objects when he could say nothing about them?

These physical objects, which may provisionally be identified with electrons (positive and negative), are not the only unities of existence discovered by the hypothetical method. Organisms present a different kind of unity. While physically heterogeneous, functionally they are units. Each part contributes to the other; while an organism is in constant interaction with its environment, and a constant interchange of material occurs with its surroundings, nevertheless it is, in its own way, a unit. While we may call the electron (provisionally) a *physical object*, we may call the organism a *functional object*. In addition, we find some minds achieving a different sort of unity, not merely a functional unity, but a unity of purpose, so that all actions contribute to this one life-purpose. While functional unity seems to be one of the conditions for the very existence of a mind, different minds achieve this teleological unity to a different degree, and a few achieve complete teleological unity. Whether such a unity is actually achieved or not, all normal persons seem to have the capacity for achieving such a unity, and manifest some degree of it. Here we find a third sort of objects, which may be called *teleological objects*. Whether there are any further sorts of objects, we do not know.[1] The existence of these three sorts of metaphysical objects seems certain: physical, functional, and teleological objects. Whether individuals of these sorts exist must be determined in each case by hypothetical induction.

§ 11. If, then, we assert the existence of an external world which is not the world of sense qualities, it becomes necessary to ask what becomes of those humanly important features of life, beauty and value? Where do they exist? Certainly they do not exist in the external world. The vibrations of air particles, with which physics deals, are certainly not the symphony that stirs me so much. On the other hand, it is equally futile to locate beauty solely in the mental world; a symphony without any air vibrations would be a complete failure to an audience! Beauty and value are just where color is—neither entirely subjective nor entirely objective—something that comes into being only as a result of the interaction of the two. The sculptor is certainly dealing with a material that is not mental in its nature; and yet the beautiful object that he creates is not merely objective; its objective characteristics are, however, the stimuli necessary to produce psychological reactions, which in turn end in the production of beauty. Beauty and value are the effect upon human beings which some external (and some internal) objects produce. They inhere in apparent objects alone.

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[1] Is God a quite different sort of external object?

§ 12. Our logical theory has broken down the barrier between metaphysics, epistemology, and natural science. Physics, biology, and psychology, deal with realities and so become metaphysical sciences as much as does epistemology. This is as it should be. Just as the advance of knowledge has already brought about the breakdown of any distinction between physics and chemistry, so the philosopher ought to have seen that all knowledge is a unit, and that the divisions between the sciences are merely human conveniences for the purposes of research and memory. The view that philosophy pursues a method different from that of natural science puts philosophy in the anomalous position of not being a science; when we recognize that her method is the same as that of natural science, we recognize that she is a science, and, at the same time, the distinction between philosophy and other sciences breaks down. Philosophy is distinct from other sciences in that it takes as its primary purpose the consideration of certain problems which they only secondarily deal with, especially the more general and fundamental problems of thought, such as "How can we know?" "What is real?" and "What is valuable?" Because of the fundamental nature of these problems, especially the problem of knowledge, philosophy may be said to be the fundamental science; but in method and in material there is no difference between philosophy and other sciences.

§ 13. It remains to ask what sort of a realism it is that we have attained by the use of the hypothetical method. If the term "realism" is interpreted in its classical sense as the theory that perception makes no difference to reality, which is the usual deduction from the realist position that the fundamental character of that which really is, is independent of its relation to the experience of a subject, then we must decline to be realists; for it plainly does make a difference to reality whether perception occurs or not. When it is possible that the energy of a sensed light wave is absorbed by chemical substances in the eye, how can we say that reality is unchanged in perception? Perception does make a difference to reality, quite apart from the fact that the percipient is himself a part of reality.

But—and here is the immense advantage of rational induction—the precise difference which perception or any other part of the knowledge process makes to reality can be analyzed out, and consequently we can determine the nature of that reality when it is not perceived. When I perceive a picture, my perception assuredly makes a difference in the real world—certain waves of light energy are stopped on my retina and part of their energy is converted into chemical excitations in certain nerves, etc.; but the difference that perception makes can be determined by the method of hypothesis. In most cases the difference that perception makes to reality (as, for instance, in the absorption of wave energy from light) is so small that it is negligible, hence the plausibility of the realist myth that knowledge makes no difference to reality. It is thus possible even in the present unadvanced state of knowledge to determine approximately what a painted picture is in and of itself, apart from my perception— a configuration of material particles described by the physicist, which is an essential part of the causal chain that acts as a stimulus to certain psychological memories, associations, emotions, desires, likes, etc., to produce my experience of the picture. This is a much more complex account of what the picture *is* than that given by either the classical idealist or the realist; but its very complexity should be an indication of its truth—the real world is so complex that we have been a long time in coming to know it, and even yet we know little of it in itself.

Perception does make a difference to reality, and the idealist is right in this respect; but perception is merely a particular case of the general law that all causal action is interaction, and that every causal process modifies all the objects concerned, at least in their relations. Where the degree of modification is very small, we may, for practical purposes, consider the object unchanged; such is usually the case; but it would be foolish and wrong to say that such is literally the case in all instances.

On the other hand, we must recognize that all sense qualities are subjective and that our present knowledge of material realities is extremely limited. Here again the idealist is right. It would, however, be totally wrong to conclude that external realities do not exist, or that they are mental in nature. So we must part company with the idealists and seek to be enrolled as realists, even though we find it necessary, in so doing, to reinterpret the meaning of realism to be the doctrine that objects external to perception *exist*, even if they do suffer change by being perceived. Perhaps we had better call ourselves rational realists, since the process of rational induction plays the central part in the knowledge of these external realities.

§ 14. In the course of this long inquiry, we have been driven hither and thither in our pursuit of a method of justifying knowledge and in the rebuttal of false opinions. We found the needed procedure in the logical method of rational induction, which consists in the assumption of a hypothesis, the deduction of a wide variety of consequences, and the checking of those consequences with experience; we found that, in a complex world, the verification of a sufficiently wide range of consequences assures us of the truth of our hypothesis. The term "sufficiently" was defined as meaning a wider range than that offered by the nearest possible alternative hypothesis. As supplementary criteria, the possibility of verifiable deductions, the principle of the non-verification of otiose elements in a hypothesis, the principle of superimposition, and prediction, were found to be valuable aids in verification. The ultimate judges of truth and falsity we saw to be those who are unprejudiced and who thoroughly understand the matter at issue. In this discovery we found a method whereby to distinguish knowledge from mere opinion.

In addition to the method of rational induction, we distinguished immediate description as that method whereby experiences are turned into conceptual statements, and empirical induction as what rational induction becomes when no range of deduction is possible, but merely the statement of a uniformity. While immediate description attains to knowledge of limited usefulness, empirical induction can never be sure of its results and so deserves the aspersions cast upon induction by logicians. An exception to a hypothesis was found not always to bring about the overthrowal of that hypothesis, but more often to point to the need of a supplementary hypothesis, and sometimes to the fact that the hypothesis is a limited universal. The progress of science consists in the demonstration of more and more general universals, which form larger and larger systems by virtue of their superior inclusiveness.

We also turned aside in our main inquiry to consider the nature of deduction. A study of the conditions of deductive rigor led us to see that all deduction is syllogistic in character, when fully expressed, and that there are other forms of syllogisms besides those which Aristotle enumerated. A syllogism consists of five and only five propositions, each of which must be enumerated and demonstrated in order to attain logical rigor. The unit of deductive inference was found to be the syllogism; immediate inference is an abbreviation of the syllogism used in dealing with familiar forms of inference. It is likewise possible to find supersyllogisms, in which more than two premises are combined. Mill's methods were seen to be inaccurately formulated canons of immediate inference for use with a causal hypothesis.

The results of this inquiry compelled us to redefine many of the terms in common use in logic, such as "logic," "syllogism," "cause," "conditions," "possibility," "impossibility," etc. We found ourselves able, not only to show that the method of rational induction can bring certainty, but that this method can be logically used to demonstrate its own correctness, which it did by accounting for the complexities of scientific method, by producing undoubted knowledge, and by bringing about a harmonious synthesis of all the various suggested methods of knowledge.

We then proceeded to apply our logical method to certain long-standing problems. By its use, we were able to solve the problem presented by the perceptual judgment, and to open a way of escape from skepticism, by building, upon the secure foundation of the indubitabilities of experience, the critical certainties of the perceptual judgment, and the critical trustworthiness of memory. Our further escape from skepticism was barred by the problem of the validity of the uniformity of nature; we found it to reduce to the principle of the eternity of truth, which was discovered to be as yet a merely empirical law, and so not absolutely certain, though the most certain of all empirical laws. Since our method of rational induction offers a method of eventually establishing the complete truth of that principle, we saw that there is no absolute hindrance to the attainment of complete certainty, and that skepticism is mortally wounded, though not dead.

Our logical theory enabled us also to determine the nature of mathematics, which was found to be twofold: pure mathematics, consisting of formal deductive systems, which possess no truth, but only validity; and applied mathematics, which possess, in addition, truth. We were also able to give a consistent objective theory of probability.

By applying the method of rational induction, we found ourselves able to transcend experience and to determine the nature of the various objects with which men deal, scientific objects, conventional objects, mental objects, and apparent objects. Scientific objects we saw comprised physical objects, functional objects, and teleological objects. We were able to demonstrate the existence of other persons and of external objects, and thus were led to rational realism, which agrees with the realism of natural science and roughly with that of the common man.

Thus our inquiry, which began with a destructive analysis of the traditional logical methods and systems, ends in the recognition of the extraordinarily fruitful and long-neglected method of rational induction—the mind's sole method to knowledge, and the definition of science.

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